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RADIO-ACTIVITY AND THE PERIODIC SYSTEM[1]

THE periodic system of the elements has for nearly half a century proved a most puzzling and absorbing problem to chemists. It has been called a law, but while there is undoubtedly an underlying law or laws, I doubt whether we have as yet any very clear conception of them. Certainly, the usual statement that the properties of the elements are periodic functions of their atomic weights was never strictly true, even in days of partial knowledge, and is much less true now. It was neither the periodicity "of the geometers," as Mendeleef himself said, nor the function of the mathematician. Indeed, we have now come to a view where, apparently, we must abandon the atomic weight as the only or even the chief determining variable.

The truth is that for many years after its announcement it was more truly a working hypothesis, and a great deal of work had to be and still has to be done before it can attain to its completed form. It contains much that is true, has been most useful as a guiding principle, and has shown a wonderful power of adjustment to new facts and increasing knowledge.

It was in 1895 that the system had to adjust itself to the first severe jolt which it received through the discovery of argon and helium, and three years later, of other inactive, monatomic elements. The necessity for readjustment here had been in part foreseen. The abrupt change in the progression of the elements from strongly electro-negative fluorine to strongly electro-

[1] Read before the Elisha Mitchell Scientific Society, March 9, 1915.

positive sodium, and, in general, the transition *per saltum* from period to period had been discussed by Reynolds and others. It needed explanation and was impossible mathematically except by passing through zero or infinity. Some, as Sedgwick and de Boisbaudran, seem to have predicted such transition elements, and when argon was discovered it was not difficult for Julius Thomsen and de Boisbaudran to arrange an entire zero group with approximate atomic weights three years before Ramsay's brilliant discovery of the other inactive gases.

There are other anomalies in the system which are difficult to explain with the accepted tabulation. Such, for instance, is the existence of the rare earths, now some sixteen in number, so closely alike chemically and so different from other chemical individuals. The more they are studied, the less possible does it seem to fit them in any vacant places in the table. Meyer has recently suggested that they may form a miniature periodic system in themselves reproducing the relations of the main system. But a more serious breakdown in the supposed fundamental principle of the system comes in the relative position of such elements as argon and potassium, cobalt and nickel, tellurium and iodine. After most exhaustive investigation of their atomic weights it has become evident that these can not be used in deciding the relative order and at the same time have these elements fall into the proper grouping with those elements chemically most nearly related to them. So the order of the atomic weights has been tacitly abandoned and the superior determining power of the chemical characteristics acknowledged. This can only mean that the mass of the atom is not the sole, nor indeed the chief, determining variable, and it would seem that the search for the latter can only be ended by the solution of the problem as to the nature of the atom itself.

Certain clews to this have undoubtedly been in our hands for a long time, but their leading was not clear and the thought of them baffling. Such, for instance, were the facts that by taking an atom of nitrogen and four of hydrogen a grouping of atoms was obtained which behaved in general as an atom and was the analogue of potassium. Or, again, carbon and nitrogen give us an analogue of chlorine—and so with compound radicals in general. But while both building and tearing down again were easy, they seemed to throw no light on how those we could not tear down were once built up.

Still another thought-inspiring fact which would seem to have important bearing on the nature of the atom and hence the meaning of the periodic system is the ease with which certain elements by a change of valence change their chemical character and form distinctive series of salts as if they had been transformed into different elements. This causes some confusion and what would ordinarily be called forcing in the present tabulation of the system, and it will be recalled that Mendeleeff, in his earlier tables, actually placed certain of the metals, as copper and mercury, in two different groups, assigning each two different places. Signs are seen in the work of Barbieri and others of a tendency to place certain of the elements in two or more different groups according to valence.

I believe that one should keep in mind the idea involved in Patterson-Muir's definition of an element as a collection or group of properties. Thus there are weight, electro-chemical nature, affinity, valence and other properties by which we recognize it and differentiate it from other elements and to which our knowledge of it is necessarily limited. There is a more or less defi-

nite gradation in these properties from element to element, showing an inter-relationship, and yet scarcely in itself justifying the conclusion that any one property determines the other or that they are dependent upon it. While it is true that it is hardly possible to dissociate these properties from some conception of matter, such conception has not yet reached its ultimate analysis and until it has we are dealing with the recognized properties alone.

In the same year in which the periodic system was forced to adjust itself to a zero group another discovery was entering upon its marvellous development which was to open up new views as to the nature of matter and radically affect the system. The remarkable and illuminating results obtained in the study of radioactive substances are paving the way for an understanding of the laws on which this system is based.

Radioactivity was regarded by Mme. Curie as an atomic property and this was the guiding thread which led to the discovery of radium. Of course, this preceded by a number of years Rutherford's announcement of his theory of successive transformation or the disintegration of the atom. It is a question whether the fact that an atom is undergoing disintegration is to be regarded as a property in the same sense as the mass, valence, etc., but so long as this change can not be induced, changed or stopped and is known to take place only in the case of a fraction of the elements it is certainly distinctive and may be called a property for lack of a better name. There is, however, undoubtedly a cause for this disintegration and this instability may be due to some inherent property of the atom.

At present there are some thirty-seven radioactive bodies known, with the possibility of still others being identified. Each has distinctive radioactive properties. For a number of these the chemical and physical properties are known. Each is an atom hitherto unknown and must be considered a new element. Of course, the present accepted arrangement of the periodic system does not provide for so many additional elements and indeed is rather hopeless for even the sixteen rare earth elements. What is to be done with this embarrassment of riches?

Soddy's study of the grouping in well-known families of a number of the better known radioactive elements according to their chemical properties, combined with a consideration of the kind of disintegration by which it was produced led him to a generalization which would enable one to place correctly any radioactive element whose source was known, and at the same time give an approximation as to its atomic weight.

Fajans arrived at the same generalization independently from an examination of the electro-chemical evidence, finding that the product of an α ray change was more electro-positive, while that of a β ray change was more electro-negative. Similar conclusions from various evidence were reached by Fleck and Russell.

The generalization is as follows:

When an α particle is expelled it carries with it two atomic charges of positive electricity and the expulsion of these two positive charges from the atom affects the valency of the product, as Fajans has pointed out, just as in ordinary electro-chemical changes of valency. If the atom were initially in Group IV., for example, its ion is tetravalent and carries four atomic charges of positive electricity. Two such charges having been expelled with the α particle, the product is in the di-valent Group II., non-separable from radium. The mass in this case is four units less. So with the β ray change. The β particle is a negative electron and the loss of this single atomic charge of negative electricity increases the positive valency of the product by one. Radium B, for example (in Group IV.), expels a β particle and becomes radium C (in Group V.). When-

ever two or more radio-elements fall in the same place in the Periodic Table, then, independently of all considerations as to the atomic mass the nature of the parent element, and the sequence of changes in which they result, the elements in question are chemically non-separable and identical. As will later appear, this identity extends also to most of the physical properties such as volatility and spectrum reactions.[2]

To express this "newly revealed complexity of matter," Soddy has suggested the word isotope. A group of two or more elements occupying the same place in the periodic table, differing in atomic weight yet chemically non-separable, is isotopic. There are possibly seven such elements isotopic with lead. Radium is one of four isotopes. The chemistry of thirty-seven radio-elements is thus reduced to a smaller number of about ten types.

Two fundamental changes in the old views as to the system are indicated here. First, the position of an element is not fixed but can be changed in either of two ways—by a change in valence (which, as is well known, can be brought about in various ways), and again by disintegration due to ray-emission. Secondly, more than one element can occupy a given position in the system. This is independent of the atomic weight, but such elements are chemically inseparable. This involves the giving up of all idea of the properties being functions of the atomic weights and necessitates the formulation of the law anew.

The place occupied by an atom is not solely determined by its mass but by its electrical content as well. According to Soddy, the system represents the chemical character of matter as the function of two variables instead of one. The electrical content is the essential variable in horizontal columns and mass is the essential in vertical columns.

It is somewhat early to raise the question as to whether all elemental atoms are the result of disintegration processes, or, conversely, of synthesis, but in any case the old puzzle remains as to their great irregularity in weight relations if the most accurate chemical determinations are to be relied upon. If the time should arrive when they could be calculated, chemists would naturally return to hydrogen as the standard. Certainly, at present these weights present no simple synthetic relations.

An explanation of this is perhaps at hand if the view of Soddy (and of Crookes at an earlier period and from a different standpoint) is accepted, namely, that in atomic weight determinations it is not a natural constant that is obtained but a mean value of non-homogeneous masses. In other words, the weight may represent the average of various isotopic atoms and not the absolute weight of identical atoms.

It is very fortunate that the simple expedient of arranging the elements in the order of their atomic weights could give the early workers so nearly correct a view of the periodic system. It would probably have remained hidden for a long time if this had not been so prominent a factor in determining the proper sequence. There is undoubtedly a proper sequence. This has been settled hitherto chiefly by consideration of the atomic weight, but also by examination into the relationship existing between the elements. For instance, the order of atomic weights would be iodine and then tellurium, but chemically tellurium belongs to Group VI. and iodine to Group VII. Therefore, the atomic weight order is reversed.

The sequence numbers of the elements, or atomic numbers as they are called, assume a new practical and theoretical importance. Within twenty years after the announcement of the periodic system, some,

[2] Soddy, "The Radio-elements and the Periodic Law," p. 6.

as Fedaroff, had sought to attach importance to these numbers, but the efforts had little to commend them. Lately it has been suggested by van den Broek that this is a fundamental and important number. Beginning with 1 for K, the numbers would be 2 for He, 3 for Li, 4 for Be, etc. The question then naturally arises, can these numbers be reliably determined without reference to the atomic weights and correcting the manifest mistakes made in following the simple order of these weights?

One method for doing so, though with limitations, lies in the measuring of the scattering of the α particles when passing through different kinds of matter. Geiger found that the angle of the scattering seemed to depend very largely upon the atomic weight of the scattering metal. A very small fraction are scattered through such a large angle that they return on the side of incidence. This deflection is, of course, both a volume and surface effect. For equal thickness of screen calculations based on Rutherford's conception of the atom and his belief that this large angle scattering is due to the near approach of the positively charged α particle to the positive nucleus of the atom of the screen would make the scattering vary as the product of the density by the atomic weight. Thus Rutherford calculated that the scattering by gold should be about fifty times that by aluminium. This has been confirmed by the experiments of Geiger and Marsden, and the relative scattering has been determined for a large number of elements. The phenomenon is manifestly one determined by the electrical content of the atom.

The nuclear charge of the Rutherford atom can be calculated from the α particle scattering at various angles. This charge is found to be one half the atomic weight multiplied by the charge of an electron. The same value was reached by Barkla by observations on X-rays. Soddy concludes that it is the nuclear charge rather than the atomic mass which fixes the position of the element, basing his conclusion largely upon the work of Barkla, Sadler and Moseley, which will be briefly outlined further on. This in reality agrees with the hypothesis of van den Broek that the number of electrons in an atom in the neutral state determines the place of the element if hydrogen has one electron and one nuclear unit charge, helium two electrons and two nuclear unit charges, etc.

The direct method then is a combination of the work of Bragg, Barkla and Sadler, and Moseley. Making use of the work of those first mentioned, Moseley photographed the spectra obtained by the cathode-ray bombardment of a number of elements, the X-rays thus produced being reflected and defined from a crystal face. The frequencies of the vibrations could be determined and this frequency was found proportional to the square of the atomic number. That is, there was a definite shifting in the direction of shorter wavelength in the spectrum of an element from that of the one next above it in the list.

The graphic representation of the system has never been satisfactory in spite of the many efforts to solve it. It is especially difficult to bring out the facts by any representation on a plane surface. The faults of the Mendeleeff table can readily be seen, and they make it very desirable to secure a better mode of expression. And yet it is difficult to use the three dimensions of space so that the average student can grasp the whole. Soddy's lemniscate curve certainly has its good points. This may be compared with the arrangement of Rydberg. It can not be claimed yet, however, that the law or laws underlying this system are known and well understood, and until such time a com-

plete and satisfactory graphic representation is scarcely to be expected. We can agree at least that progress is being made toward such an understanding.

FRANCIS P. VENABLE

SOME FALLACIES IN THE ARGUMENTS AGAINST FULL-TIME CLINICAL INSTRUCTION[1]

IN a recent paper, published in SCIENCE, Dr. S. J. Meltzer comments upon two notable facts in connection with the present rather active agitation regarding full-time clinical instructors. The two facts singled out by him are: (1) The appointment of full-time professor of medicine, surgery and pediatrics, by the Johns Hopkins University, and (2) the disparagement of this type of plan by the council on medical education of the American Medical Association. Dr. Meltzer's paper itself constitutes a third notable fact, in that it represents one of the very few unqualifiedly strong appeals that have been made by a clinician in favor of full-time clinical instruction. Although engaged at present in a so-called fundamental research, the current of Dr. Meltzer's life has been clinical to so large a degree, that his conclusions can not be questioned on the ground of academic impracticability. He analyzes the report of the council with logical seriousness; and were it not for the artifice of a single italicized word, one would scarcely feel the flick of Meltzer's lash or realize the seriousness of the attempt of the council to laugh the case out of court. Dr. Meltzer, by rare grace and tact, forges an argument so uncommonly well tempered as to render supportive discussion almost unnecessary. And yet, if there be any force in the plea for full-time heads of clinical departments, it lies in the line of duty of those of us who are clinicians to develop its full strength by discussion.

In such a discussion, as indeed in all such discussions, nothing contributes so much to balance and rationality as does a proper conception of the historical perspective of the problem involved. It is essential to realize at the outset that the question is not a new one involving American medicine alone. Many men would have us believe that suddenly, as a result of this, that, or the other tendency, our clinical instruction in America has been found wanting, and that with typical American impulse we have set to moving in the sacred realm of education, the machinery of experiment. As early as the seventeenth century, Leibnitz attempted to justify his faith in quacks, on the basis that doctors were improperly trained as men of science, and that it was hopeless to look for the development of scientific teachings and methods in a practitioner, *der nichts thut als von einem Patientem zum andern rennen, und wenn er bey dem einen ist, auff den andern schon denket* (who does nothing but run from one patient to another and who, when he is visiting one patient, is already thinking about the next one). Almost a half century ago Billroth anticipated the Flexner report on Medical Education, in his "Ueber Lehren und Lernen," a work necessarily less modern in tone than Flexner's, less broad in the geographical consideration of the subject, but not a whit less emphatic in the assertion of corrective principles. Coming down to more modern times, we have the Report of the Royal Commission on University Education in London (1913) in which it is admitted that "the academic training received by medical students in London has not always been distinguished, and that the scientific spirit has been too often wanting." We in America have also found that, even in our best schools of instruction, the scientific spirit has been too often wanting, and we have found it wanting chiefly in the clinical branches. On this basis rests the agitation for full-time clinical instruction.

The phrase "full-time clinical instruction" signifies that the teaching of each major clinical subject be under the supervision of a properly qualified instructor, who shall serve as the head of his department, who shall devote all his energies during the working

[1] Read before the twenty-fifth annual meeting of the Association of the American Medical Colleges, Chicago, February 17, 1915.

school-day to the management of his department, who shall receive an adequate compensation for his highly specialized labor, and who shall be protected against the inevitable lures and enticements incident to his position, by a provision which denies him the right to accept private fees, or permits him to accept them only on such conditions as may be imposed by the university. This is the simple statement of the case. And as the question stands at present, its importance resides not in the working out of a detailed scheme of clinical instruction under such a plan;[2] but rather in formulating a critical judgment regarding the advisability and practicability of so modifying our method of clinical instruction as to make it conform to other approved methods of education.

And when we have said this we have hinted at one of the most paradoxically inexplicable phases of medical education. It may be stated that, almost without exception, *clinical* teachers realize the essential necessity for full-time men in all of the *fundamental* branches of medicine. The very canons of education demand such a system. Yet, a large number of these same clinical teachers assume that there is such a wide divergence between the teaching of the fundamentals and of clinical medicine, as to render wholly unwarrantable the conclusion that clinical teaching also should be based on that plan which alone is best suited for instruction in fundamentals. It is, for very self-evident reasons, natural that the scheme for full-time clinical instruction should have the strong support of most of the teachers of the fundamental branches. It is not so easy to explain the fact that opposition to the plan has come so largely from clinicians. Such a clean-cut division into camps is unfortunate, because it has set in motion a controversy tinctured with bitterness. The so-called laboratory men are charged with a tenacious hold on impractical ideals, limited by virtue of a narrow occupational horizon; and the clinicians are, in their turn, supposed to typify the old story, repeated in myriads of forms, of privilege clinging to tribute. Neither of these assumptions is entirely correct; both of them are essentially harmful because they drag the argument down to the low level of personalities. Disagreements of this sort usually rest on fallacious judgments. An unqualified advocate of the full-time clinical instructor, I have, for the past few years, noted various fallacies, patent or concealed, in the arguments against this plan of instruction; and the only object of this contribution is to examine these various fallacies, with the hope of clarifying a fairly well-confused topic.

Of all others, the fallacy most responsible for both bitterness and confusion is the assumption that full-time clinical instruction connotes a clean sweep, displacing all teachers who are private practitioners and replacing them by non-practitioners. Such a plan has the advocacy of no one. Barker, in his address on "Tendencies in Medical Education," falls into this particular fallacy when he develops the thought that "the present incumbents of clinical chairs" by virtue of "the rightfulness of the kind of work done by them" hold their positions in "good faith." He pleads the cause of these "honest, hard-working men" in such fashion as to warrant the inference that they are all to be displaced, and that their displacement is a breach of moral contract on the part of the university. Dr. Barker certainly does not, nor should any one else, minimize the value of such services as are rendered at Johns Hopkins University, for example, by those clinical men who are not on a full-time basis, simply because at that university there are academic heads to medicine, surgery and pediatrics. It is supremely important to recognize the fact that the varying character of clinical material will always make it both advisable and necessary for the university to offer place and preference to the properly qualified clinical teacher, irrespective to his affiliation with private practise. The full-time clinical instructor, together with his staff, is a necessary adjunct in organizing, coordinating and correlating

[2] Details of organization are purposely omitted, such, for example, as the number of full-time salaried assistants necessary to the successful conduct of a department.

the practical as well as the investigative work of his department, just exactly as the dean of a school is an adjunct in developing school spirit and school policy. The advocates of the full-time instructor should never, not even implicitly, subordinate the teaching value of the properly qualified private practitioner.

Even broader in scope is the fallacy that there is an important and essential variance of principle in teaching the clinical phenomena of disease, and in teaching function and structure or aberrations of both, in the laboratory. It is difficult to analyze this fallacy and at the same time avoid an undesirable discussion of the primary pedagogic principles involved in teaching medical students. It may be pardonable, however, to dip into abstractions just deeply enough to say that whether our efforts at teaching be confined to the fundamental or to the clinical branches, our aim is toward equipping our pupils to form proper judgments. If, as a result of their training, our students can affirm or deny conclusions, either by proper process of reasoning or by the direct comparison of objects to ideas, we may rest easy in the thought that the discipline of their medical education has been fruitful. And the process by which they should be taught to form proper judgments is exactly the same in the hospital ward as it is in the laboratory. In both places the student is taught to know certain fundamental truths, and from these he is taught to reason certain definite conclusions. The fact that in so many hospital wards and clinic rooms the student is taught *to know,* to the exclusion of being taught *to think,* is responsible, in large measure, for the fallacy that clinical teaching is, part and parcel, separate and distinct from fundamental teaching. If one doubts that clinical teachers err with hopeless frequency in this direction, let him pick up at random a number of clinical text-books and examine them critically. The conclusion will be unavoidable that preponderant stress and effort is laid on crowding the student with facts—on teaching him to know. One of the most recent clinical text-books states in its preface that the very best a teacher can hope to do is to teach his student to know.

This particular fallacy regarding the specific difference between fundamental and clinical teaching should not be dismissed by merely stating it. It is essential to expose the danger to which it leads. And this can be done no better than by quoting a sentence from last year's report of the Conference on Medical Education. This report states that

> Clinical teachers know that in the very nature of things the teaching of anatomy and pathology is in no way parallel to the teaching of medicine and surgery, because the teaching of medicine and surgery is inseparably associated with the practise of medicine and surgery.

This allows us absolutely no other alternative than the conclusion that anatomy and pathology are *not* inseparably associated with the practise of medicine and surgery. Surely the council can not hope that this conclusion will go unchallenged.

On the part of the clinicians there has always been a tendency to introduce this notion of the subtle, specific teaching value of private practise as a sort of abracadabra, charm, amulet, something to conjure with in the realm of medical education. They have studiously avoided the fact that the plan for full-time clinical instruction contemplates developing the principles of practise in their most utilizable form, namely from a variety of clinical material, intensively correlated and studied, and housed under one roof. Is there more to be learned of the basic traits of human nature on Fifth Avenue, or on Michigan Avenue, than there is in the wards of Bellevue or of Cook County Hospital? Or does the wealthy patient have a more legitimate demand on a larger share of the sympathy, interest, pity, or sweetness and light of his doctor's pervasive personality than does the helpless sufferer in the charity ward? The plan for full-time clinical instruction *does* contemplate the full realization of the intimate relationship between teaching medicine and practising medicine; what it does *not* contemplate is the injudicious mixture of private practise and teaching. And in this particular, the plan is strong against all

attack or argument, for the very reason that the majority of clinicians do not (and very properly do not) use their private patients as teaching material and could not even if they were so minded.

And all this leads up to another false assumption. It is argued that since from the standpoint of medical education, so little store is laid by a man's capacity to gain and hold the medical confidence of a large clientele, and to serve it intelligently and well, it necessarily follows that the rôle played by the private practitioner is less ennobling than that of his fellow who elects to be exclusively a clinical teacher. The practising physician very naturally resents such an inference. In reality, any conclusion which sets a comparatively lower value on the services of the private practitioner than on those of the exclusive clinical teacher, by reason of the fact that material remuneration is greater in one field than in the other, is a *non sequitur*. Certainly all thinking men realize that between the *spirit* of practise and the *spirit* of teaching there is no essential ethical difference. The value of effort in either field is directly proportional only to the grade of intelligence and purpose back of it. But between the *demands* of practise and the *demands* of teaching there is a variation so pronounced, qualitatively and quantitatively, as practically to preclude the proper performance of both these functions by the same individual. The full-time plan, therefore, rests upon this very rational conception of the case, and implies absolutely no measure of comparative worth between the vocations of practitioner and teacher.

In the teaching of such eminently practical branches as law, engineering, commercial chemistry, and other technical specialties, the need of the full-time instructor has been recognized and met. There seems to be nothing specifically so different in the practise of medicine as to demand that it be regarded as an exception in the general field of education. On the contrary, the teaching of clinical medicine demands the services of unattached men more urgently than does the teaching of any other practical art or science, because the two purely physical elements of time and fatigue enter so intimately into the problem. Barker has emphasized the overwhelming amount of correlated knowledge to be appropriated by the clinical teacher of to-day; an amount of data almost sufficient "to suffocate" him. This process of appropriation requires, in addition to intelligence, a very definite number of hours and minutes each day. An active practise rarely grants the necessary surplus of time. If, however, by a process of "speeding up," the practitioner succeeds in cleaning his slate, in order to fulfil his teaching obligations, he is very apt to find himself face to face with that other disturbing physical element—fatigue. It has always seemed a remarkable fact that the study of fatigue in its relation to efficiency should have been confined to the industries. We accept as true the fact that more than a given number of hours in his cab renders the locomotive engineer an unsafe person to differentiate between the two primary colors red and green; but we have to prove by argument that the busy surgeon can shoulder the enervating duties that confront him day and night, and still be fit for one of the keenest of all mental disciplines—the proper teaching of science.

And let us pause here just long enough to emphasize this word science in its relationship to clinical medicine. Not the least significant of the various fallacies that we are examining is the one that has to do with the thought that the fundamental man *must* be a specialist, and *must* be on a full-time basis because, although of course he is a teacher, he is also an investigator and must therefore have the necessary time for *scientific* research. By inference again we are subtly led to believe that scientific research is confined to anatomy or physiology or one of the other cognate fundamental branches of medicine, and that it need not be reckoned with in considering the teaching of the clinical branches. Those who favor the plan of full time clinical instruction are influenced in no small part by the hope that the properly qualified clinical teacher, favorably situated, will foster, stimulate and direct scientific clinical research of a higher order than is commonly produced under our present system of conducting clinical teaching. Clin-

ical investigation is, of all other types, probably the most intricate and difficult, for the reason that the problems studied are of such a nature that the factors entering into them can not, as a rule, be varied at the will of the investigator. If, therefore, we hope to encourage worthy product along the lines of scientific clinical research, we must, to say the least, provide the clinical teacher with an environment as favorable as the one with which we surround the fundamental teacher. It is no answer to this argument to quote the numerous examples of epochal discoveries made by busy practitioners. The superman will inevitably enrich his field, in the face of compromising odds or even of grueling adverse conditions. The problems of education always deal with averages, and what we desire to see is a system attuned to producing from among the common ranks of medical men a proportionately large number of clinical teachers and investigators.

We base our hopes on the full-time plan as an aid in attaining this worthy end, and all seems well until we are rudely halted by the oft-cited example of Germany, the nourishing mother of all that is best, and stable, and approved, in medical education. Germany has no full-time clinical instructors, and, what is more, the very men whom we all recognize as her leading clinical educators have not a particle of sympathy with the American full-time plan. Here truly is a stumbling block. And yet, the explanation is not as difficult as it appears to be. German clinical teachers, in spite of their unqualified rights to practise, have mortised themselves into medical history, so that their names fairly dot pages. More than that, practically every great German clinical teacher has developed about him a so-called school of younger men. By contrast, we have at home a proportionately very small number of names that even the most chauvinistic among us would set up with the leaders of German clinical thought, and only comparatively few of our clinical teachers have grouped a school of enthusiasts around them. But this contrast does not signify that the German clinical professor is efficient because of his uncompromised right to practise. At all events, it would be difficult to establish proof to this effect. It seems much more likely that he is efficient in spite of the fact that he shoulders the distractions of practise. Indeed, those who have come into intimate contact with the directing heads of clinical departments in Germany know that many of them resolutely set themselves against these distractions. Friedrich Mueller, of Munich, may be selected as a type. Mueller considers his two-hour *sprechstunde* devoted to private patients, as a type of relaxation, comparable to golf, mountain climbing, or other forms of diversion. No inducement will persuade him to lengthen the office hour, and he refuses to make extra-urban visits, under ordinary circumstances, unless there be some teaching value inherent in the call. His serious work is teaching and directing, to both of which he devotes consummate care, and consequently a large amount of time. Between Mueller as a teacher of medicine and, let us say, Marchand as a teacher of pathology, there is no essential difference. They are both so-called fundamental men, each in his own specialty; and Mueller represents the type that the advocates of full-time instruction in America hope to develop—the fundamental clinician as teacher.

If we be asked why we concede that private practise has not militated against the development of the highest type of clinical teacher in Germany and has so markedly militated against it in America as to call forth an edict of interdiction, we can answer only that the variance between German and American culture and traditions so profoundly influence thought and act as to render it impossible to graft, unaltered, a system of thought from one country to the other. It is likewise equally impossible to argue that because certain conditions are favorable from an educational point of view in one country, they must of necessity be favorable in the other. The German is the type of patient plodding lover of *gemuetlichkeit,* who, certainly up to recent times, did not labor in medical fields under a very heavy stress of commercial competition. Tradition requires that he advance to scientific preferment only through a dozentship, and this in turn implies approved excellence as teacher or producer. The American, on

the other hand, is the mercurial, restive type, who hasn't even a word in his vocabulary with which to translate *gemuetlichkeit,* and who labors medically in a strenuously competitive atmosphere. The essence of the matter is simply this, that up to now the German clinical professor has, as a rule, needed little or no protection against himself, whereas the American clinical professor has so frequently demonstrated the need of such protection as to call forth that forcible truth from Dr. E. P. Lyon, who characterized clinical professorial selfishness by the phrase "lying full length in the trough as he eats." If a sufficiently large number of American private practitioners had demonstrated their capacity to combine teaching and practise as the Germans combine them, there would probably be no call for the full-time clinical professor. They have failed to demonstrate this, and they can not explain that failure on the basis of German example.

Indeed, this failure on the part of the clinical teachers to teach as intensively as do the instructors in the fundamental branches is alone responsible for the agitation for the full-time clinical instructor. Whether they accept it or not, the burden of proof lies upon those who argue against a plan that attempts to do for clinical teaching exactly what has been recognized as essential in practically every other branch of education. For many of us it is difficult to see how the introduction of full-time clinical instruction can possibly fail to accomplish most of those things which we hope to see result from it, for all of us who are interested in seeing the reform meet with warm, broad support, there is much chagrin and disappointment in contemplating the half-hearted support and whole-hearted opposition accorded it. This chagrin and disappointment may be considerably tempered, however, if we bear in mind the truism spoken by President Lowell in his address before the New England Association of Colleges, last year. Said Mr. Lowell:

> Education is the last of all things to follow the stream of human thought and progress. It is still mainly in the deductiv stage.

If Mr. Lowell be correct in his statement, we may seek solace in the thought that we have at least an explanation for the fact that so many well-meaning clinical men experience difficulty in accepting an inductive syllogism the conclusion of which is "The teaching of clinical subjects should be under the guidance of exclusive clinical teachers."

MAJOR G. SEELIG

ST. LOUIS UNIVERSITY SCHOOL OF MEDICINE

CHARLES E. BESSEY

THE death of Professor Bessey removes a conspicuous figure from among the group of older American botanists. No botanist was better known personally among his colleagues. for he was eminently social, and enjoyed the various scientific meetings that brought his friends together. It is certain that no member of the botanical fraternity will be more missed at these meetings than Professor Bessey, for he was always the center and life of any group of which he happened to be a member.

The usual biographical data dealing with birth, training and official positions may be obtained from "American Men of Science," and need not be repeated here. The writer wishes to speak of him as an old acquaintance, and of his place in the history of American botany.

Professor Bessey first became known to botanists in general in connection with his position in the Iowa Agricultural College at Ames, and during his fourteen years (1870–84) of service there, his reputation as a botanist became established. In 1884 he began his long period of service at the University of Nebraska, where for thirty-one years (1884–1915) he was not only a commanding figure in his subject, but also in the university and in the state.

In the history of American botany, Professor Bessey stands for the introduction of a new epoch. Before 1880 the study of botany was practically bounded by the taxonomy of the higher plants, with such gross morphology as enabled the student to use a manual. In any event, the collecting and naming of plants was the chief botanical pursuit. For nearly thirty years before 1880, morphology as we understand it now had been developing in Germany, under the original stimulus given

by Hofmeister. The belated introduction of American students to this new field of botany was brought about by Professor Bessey, when in 1881 his "Botany" appeared. This volume not only brought the atmosphere of Sach's *Lehrbuch* to American colleges, but also compelled the development of botanical laboratories. For the first time, all plant groups became available, and cells and tissues became materials for study. The original "Botany" was the first of a long series of texts, and for many years "Bessey's text-books" set the standard for modern work. If Professor Bessey had made no other contribution to American botany than the publication of this book at the psychological moment, he would have made for himself an enduring place in the history of American botany.

The qualities that led him to discover and introduce to American colleges the new botany, also suggest that he was a great teacher. Perhaps no American botanist has left his mark on so many students as did Professor Bessey. He was certainly "apt to teach," and this was shown not merely by his neverfailing enthusiasm for his subject, but also by his stimulating companionship with his students. He lived in his subject and lived with his students, and his "dingy and cramped quarters," as they were called, seemed to cultivate the spirit of camaraderie in the whole department. The students of Professor Bessey are scattered everywhere in responsible positions, and the writer has never met one of them who has failed to pay the warmest tribute of loyal affection to the man who taught him.

Professor Bessey was not merely a great teacher, both through his text-books and in contact with his students, but he was also a public-spirited citizen. He felt that the whole state of Nebraska was entitled to his services, and he gave of his time freely to organizations of all kinds that were seeking to develop the various interests of the state. The plant life of the state, the agricultural possibilities of the state, the teaching of agriculture in the schools, all engaged his attention.

Recognition of Professor Bessey by his colleagues throughout the country came as a matter of course. He was not only a member of the various national organizations, but he was elected to almost every office to which an American botanist can aspire, culminating in the presidency of the American Association for the Advancement of Science. One of the characteristics of Professor Bessey most frequently remarked among his colleagues was his refusal to speak unkindly of any one. No one ever heard from him the sharp and occasionally envious criticism that too often mars the fine qualities of scientific men. Even in his work as a reviewer, where criticism is invited, he always searched for the pleasant things to say, and left the unpleasant things unsaid. Those of us who knew him best realize that he did not even think of the unpleasant things, but that his kindly nature was always seeing the good in every botanist.

Professor Bessey was a voluminous writer, as a man full of ideas, energy and of the teaching spirit is apt to be, so that it would be impossible to cite his bibliography here; it will doubtless appear in fitting form in some more appropriate connection. The present purpose is simply to express an appreciation of a great teacher of botany by a colleague who has known him intimately throughout almost his entire public career.

John M. Coulter

FRANK OLIN MARVIN

Professor Frank Olin Marvin, dean of the school of engineering of the University of Kansas, died in San Diego, Calif., on February 6, 19155. Dean Marvin was born in Alfred Center, N. Y., in 1852. He was the son of Dr. James Marvin, for many years professor of mathematics in Alleghany College, and later chancellor of the University of Kansas.

Graduating in 1871 at Alleghany College, Professor Marvin devoted several years to practical engineering work. and was in 1875 appointed instructor in mathematics and physics at the University of Kansas. In 1883 he was appointed professor of civil engineering, and when, in 1891. the university was reorganized and a school of engineering was established he was elected to the position of dean. He was untiring in his labors for the upbuilding of this most important school, from this

time until 1912, when impaired health compelled him to retire from active work, although he was retained on the faculty as advisory dean. Last year he was granted a retiring allowance by the Carnegie Foundation.

It may be truthfully said that Dean Marvin devoted his life to the cause of engineering education. He worked and wrote for its advancement. In 1901 he was elected president of the Society for the Promotion of Engineering Education. He was one of the charter members, and the first president of the Kansas chapter of the Society of the Sigma Xi, one of the earliest chapters of this organization established. He was honored with the presidency of the national organization, and did much to shape the policy and raise the standard of this society.

As an active member of the American Association for the Advancement of Science (vice-president in 1896); of the American Society of Civil Engineers; of the Society on Testing Materials; Kansas Academy of Science; and as advisory member of the Kansas State Board of Health, he took an active part in the work for the encouragement of research and the advancement of scientific knowledge.

His colleagues in the university and the thousands of students who have been under his instruction, feel that a friend has gone. In the words of one of Dean Marvin's former students:

> He was further qualified for his work by his culture and refinement. No man was better fitted than Frank Marvin to plant in his boys the desire for the fine things of life. He was a reader, a student, an artist. Through all the busy years of striving and building, of creating great properties, or of humble service in some of the quieter places in life, Frank Marvin's boys look back to the school days of long ago and recall the quiet cultured gentleman who gave them so many ideals and who in his own life so lived these ideals.

The University of Kansas has honored the name of the first dean of its engineering school by naming the new engineering building "Marvin Hall," and the former students and friends are about to place in the building a bronze bust to commemorate his name.

Lawrence, Kans. E. H. S. Bailey

THE CHEMICAL INDUSTRY IN GREAT BRITAIN

The position and prospects of the British dye industry were discussed by Dr. W. H. Perkin, Waynflete professor of chemistry, Oxford, in his presidential address delivered on March 25 at the annual general meeting of the Chemical Society, London. Dr. Perkin is the son of the late Sir William Perkin, F.R.S., the discoverer of aniline dyes. "The Position of the Organic Chemical Industry" was the title of the lecture, and Dr. Perkin according to an abstract in the London *Times* at the outset expressed his conviction that the causes of the decadence of the industry in this country were still imperfectly understood. One of the main reasons for our present position was that we, as a nation, and our manufacturers in particular, had failed to understand the extreme complexity of the scientific basis of organic chemical industry. The decadence of the coal-tar industry and its gradual transference to Germany began during the period from 1870 to 1875. It was in 1874 that the works of Perkin and Sons at Greenford Green were sold to the firm of Brooke, Simpson and Spiller, and these works were then much in advance of anything that existed in Germany. One reason for the sale, Dr. Perkin said, was his father's natural dislike to an industrial career, and his desire to devote himself entirely to pure chemistry.

There was, however, a much more weighty consideration. It was recognized that the works could not be carried on successfully in competition with the rising industry in Germany unless a number of first-rate chemists could be obtained and employed in developing the existing processes, and more particularly in the all-important work of making new discoveries. Inquiries were made at many of the British universities in the hope of discovering young men trained in the methods of organic chemistry, but in vain.

The value of the coloring matter consumed in the United Kingdom was £2,000,000 per annum, and these dyes were essential to textile industries representing at least £200,000,000 a year and employing $1\frac{1}{2}$ millions of workers, and

to many other industries such as the wall-paper, printing and paint industries requiring lakes and pigments.

In 1870, the time when this industry commenced to be transferred to Germany, organic chemistry was not recognized by our older universities, and the newer universities, which since then had done so much for the progress of science, did not exist. Many of our universities and particularly those of Oxford and Cambridge, and those in Scotland, contributed practically nothing to the advancement of organic chemistry in the latter part of last century, and even now their output of research was far less than it should be. In Germany, as soon as the importance of the subject became apparent, schools specially devoted to the subject were founded by such teachers as Liebig, Wöhler, Kekulé and Baeyer.

The president then dealt with the deficiency of dyes in this country, and referred to the schemes proposed by the government to ascertain the best means of obtaining sufficient supplies of chemical products. The grant of £100,000 which the government proposed to make to the company for research purposes would be better employed in subsidizing the research laboratories of those universities and colleges which were willing to specialize in organic chemistry, and to train a certain number of students with a view to their entering the services of the company. The existing dye works in this country compared very unfavorably, he said, with those in Germany, where experience had been in favor of building large works and against spreading manufacturing operations over small works situated in different parts of the country. Moreover, in the manufacture of any substance, by-products resulted which must be either recovered or used in the manufacture of other saleable products, and in order that these by-products might be used to the best advantage the dovetailing operations should be carried out on the same site, and thus save transporting the by-products from one works to another—an operation that must entail loss. The proposal of the government, therefore, to take over the existing works in this country appeared a doubtful policy.

INTERSTATE CONFERENCE ON CEREAL INVESTIGATIONS

The undersigned committee on arrangements respectfully announce that on May 25–28, 1915, an Interstate Conference on Investigations of Cereals will be held in California. This proposed conference is the outgrowth of suggestion and expressed desires on the part of many investigators for a number of years that such a conference be held for the purpose of conferring on the various phases of all cereal research but particularly those more difficult problems concerning which there is difference of opinion, different methods of work, different points of attack and considerable variation in results, in order that these differences may be better understood by each other and that all such investigations be more coordinate and effective hereafter.

It seems that the fact that there are no other meetings at that time to conflict with this one and the fact that it is near harvest time in that region would be much in favor of having the conference at the time stated. It is realized that the time may be inopportune for some and that many in the eastern part of the United States will not be able to attend. It is hoped, however, that even of those in the east there may be certain ones who would in any case visit California about that time and who would avail themselves of the opportunity to take part in the conference. It is hoped and expected that there will be a good attendance from the territory west of the Mississippi River. A number have already signified their intention of being present and several have submitted titles of papers.

The arrangements are: To meet at Merced Tuesday, May 25, for a field inspection of the San Joaquin Valley cereals, go to Berkeley the evening of the same day and begin the conference proper the morning of the 26th at the University of California; continue the program the next day at the State Experiment Farm at Davis and finish the day with an inspection of the farm; then go to Chico in the evening or the next morning and visit the Plant Introduction Garden of the United States Department of Agriculture on the 28th. During the same day those who wish will go

by automobile to Biggs to inspect the Rice Experiment Farm at that place. That day will end the conference, after which the individual delegates will spend such other time and go to such other points within the state as they desire.

The following are proposed as general subjects for discussion, under each of which such subtopics may be discussed by different members as their inclination may dictate:

1. Problems of Pacific coast wheat production.
2. Improvement of barley for the Pacific coast.
3. Problems in cereal smuts.
4. Grading, milling, malting and baking.
5. Weed control in cereal production.
6. Tillage and crop rotation.
7. Insect enemies of cereals.

A program in detail will be issued later. It is expected that the discussions will cover a broad field. Millers, malsters and other dealers in grain, as well as agronomists, pathologists, chemists and entomologists are expected to attend. Among the foreign investigators expected to be present is Dr. F. Kølpin Ravn, professor of plant pathology at the Royal Landbohøjskolens, Copenhagen, Denmark. It is requested that each one expecting to attend make the fact known at once to some member of the committee. Titles of papers should be sent to M. A. Carleton, Department of Agriculture, Washington, D. C., and any other communications of inquiry concerning arrangements for meetings and other local information to Dr. J. W. Gilmore, University of California, Berkeley, Cal.

J. W. Gilmore,
M. A. Carleton,
F. S. Harris,
Ralph E. Smith,
F. D. Heald,
L. A. LeClerc,
F. M. Webster,
Committee

THE HARPSWELL LABORATORY

The Harpswell Laboratory, which has been maintained for several years at South Harpswell, Maine, as an institution of research, has been incorporated under the laws of Maine and has been placed in charge of a board of ten trustees. According to its charter, it is to be devoted to scientific study and investigation, while its constitution provides that institutions contributing funds to a specified amount will be entitled to appoint a trustee to represent its interests in the laboratory, the remaining trustees being elected annually by the corporation. The membership of the corporation includes those who have conducted investigations there and who have paid annual dues of one dollar for the current year.

Since its establishment, the laboratory has afforded facilities to 79 different individuals who have carried on investigations there. These have represented 43 institutions of learning. There have been published as based wholly or in part on investigations in this laboratory about sixty papers, making a total of about two thousand pages, illustrated by many figures and plates.

During the last season fourteen persons carried on research at the laboratory, these coming from Tufts College, Wellesley College, Wistar Institute, Johns Hopkins University, Northwestern University, Washington University and the University of Illinois. Their investigations covered: The exact homologies of the somites in the lower vertebrates, origin of taste-buds in Elasmobranchs, the nerves of the electric organs in skates, the morphology of the lungs and airsacs in birds, structure and functions of the ampullæ of *Lorenzini*, the development of the Piperaceæ, structure and development of the epiphysial organs of the dogfish, early development of *Clava*, development of liver and pancreas of *Acanthias*, the morphology of the hypophysis of Elasmobranchs and the skull of the dogfish.

The most important addition to the equipment for the year was a motor boat, 26 feet long with a two-cylinder, ten horse-power engine which puts all parts of Casco Bay within easy reach. The boat which had served for ten years was too small and had developed some of the infirmities of age. The library has been increased by several gifts and now contains over a thousand volumes and

pamphlets devoted to biology. It has several complete series of journals and some others of which only a few volumes are lacking.

The most imperative need of the laboratory is a new building capable of accommodating twenty investigators at one time, with a practically fireproof part for the library and valuable apparatus. Another necessity is a larger income. At present the laboratory is supported by subventions from several institutions. For several years the work has been carried on at an expenditure of less than $500 in any one year. Out of this small sum a collector has been employed, the absolutely essential supplies have been bought and some additions have been made each year to the permanent equipment.

During the coming season the laboratory will be open from about June 20 to September 10. It offers especial facilities for the embryology of the fishes and for experimental work on that most favorable material, the eggs of *Cerebratulus* and of *Echinarachnius*. The more northern fauna marks the laboratory off from similar institutions farther south, while the location assures one of a cool summer. No instruction is given, but the facilities are offered free to those competent to carry on investigations. All communications as to places in the laboratory as well as to accommodations in the town should be addressed to either Professor H. V. Neal, Tufts College, Mass., or to J. S. Kingsley, Urbana, Illinois.

SCIENTIFIC NOTES AND NEWS

DR. J. GEORGE ADAMI, professor of pathology in McGill University, Montreal, has left for England to take up work as a member of the British War Office, having charge of the preparation of a medical history of the war.

DR. S. ALFRED MITCHELL, formerly assistant professor of astronomy at Columbia University, and now director of the Leander McCormick Observatory at the University of Virginia, has been appointed Ernest Kempton Adams Research Fellow by the trustees of Columbia University.

OWING to the illness of Dr. Theobald Smith, the dinner which was to be given in his honor at the Harvard Club, Boston, on April 17, has been postponed until June.

THE Medical Club of Philadelphia announces a reception to be given at the Bellevue-Stratford, on April 23, in honor of Edgar Fahs Smith, LL.D., provost of the University of Pennsylvania; Alba B. Johnson, Esq., for the president of Jefferson Medical College; David Milne, Esq., president of the Medico-Chirurgical College of Philadelphia, and Russel H. Conwell, D. D., president of Temple University.

DR. EDMUND B. WILSON, Da Costa professor of zoology at Columbia University, has been appointed by the trustees to be speaker at the opening exercises of the university, on September 22.

THE van't Hoff fund committee of the Academy of Sciences of Amsterdam has awarded $120 to Dr. E. D. Tsakalotos, of Athens, in aid of his researches on the thermal properties, the viscosity and the magnetic susceptibility of binary mixtures, capable of yielding endothermic compounds.

THE Academy of Sciences at Vienna has allowed $200 to Professor H. Dexler, of Prague, to aid in continuing his studies on stimulation of the brain cortex in the horse, and $150 to Dr. E. Pernkopf, of Vienna, to aid in his study of the development of the intestines and omentum.

DR. GEORGE SARTON, editor of *Isis,* who was compelled to leave Belgium with his family on account of the war, has accepted a lectureship at George Washington University. Dr. Sarton will lecture on the history of science. At the close of the war, it is his intention to return to Belgium and resume the publication of *Isis*.

THE Longstaff medal for 1915, of the Chemical Society, London, has been presented to Dr. M. O. Forster, F.R.S.

THE Samuel D. Gross prize of the Philadelphia Academy of Medicine for the year 1915 has been awarded to Dr. John Lawrence Yates, of Milwaukee, for his essay entitled, "Surgery in the Treatment of Hodgkin's Disease." The amount of this prize is $1,500.

Professor W. Winternitz, of Vienna, known as the founder of scientific hydrotherapy, celebrated his eightieth birthday on March 1.

Professor Lillien J. Martin, of Stanford University, has undertaken the chairmanship of the committee of the American Psychological Association appointed to arrange for and conduct the program of psychology to be held at San Francisco during the first week of August. The committee otherwise remains as previously announced, the additional members being Professors G. M. Stratton and Warner Brown, of the University of California.

Dr. W. H. Manwaring, of Stanford University, has been appointed chairman of the pathological section of the National Association for the Study and Prevention of Tuberculosis, that will meet in Seattle, Wash., from June 14 to 16.

In accordance with a provision in the annual Naval Appropriation bill, President Wilson has appointed an advisory committee on aeronautics. The purpose of the committee is to map out plans for stimulating aviation in the army and navy, and to adopt the best measures for overcoming the relative weakness of the United States military services in this field. The committee is composed of Brigadier-General George P. Scriven, chief signal officer, U. S. A.; Lieutenant-Colonel Samuel Reber, aviation section of the Army Signal Corps; Captain Mark L. Bristol, U. S. N., in charge of the Naval Aeronautic Service; Naval Constructor Holden C. Richardson, U. S. N.; Dr. Charles D. Walcott, secretary of the Smithsonian Institution; Charles F. Marvin, chief of the Weather Bureau; Dr. S. W. Stratton, chief of the Bureau of Standards; Byron R. Newton, assistant secretary of the Treasury; Professor W. F. Durand, of Stanford University; Professor Michael I. Pupin, of Columbia University; Professor John F. Hayford, of the College of Engineering, Northwestern University, and Professor Joseph S. Ames, of the Johns Hopkins University.

Professor G. D. Harris, of Cornell University, will repeat this summer the tour which he took last summer. The trip will be made in the motor-boat *Ecphora* and will cover approximately the same territory as last year. The party will leave Ithaca early in June and will consist of Professor Harris and the six or seven graduate students who intend to make geology their life work. The route chosen takes an inland course down the Atlantic coast, planned in such a way that the geologists can study the different rock systems of the geologic column. From Cayuga Lake the party will enter the Erie Canal *via* the Montezuma Canal, proceed to Albany and thence down the Hudson to New York, cross New Jersey by the New Brunswick Canal and reach Chesapeake Bay through the Delaware River and the Delaware-Chesapeake Canal. The last part of the journey will be a tour through the canals of the Dismal Swamp, and the trip will end in the vicinity of Wilmington, North Carolina.

Dr. Allen W. Freeman, Richmond, Va., has resigned as assistant state health commissioner to become epidemiologist for the U. S. Public Health Service at Washington.

Stuart P. Miller, graduate assistant in the chemical department of the Massachusetts Agricultural College, has accepted an appointment with Parke, Davis and Company, of Detroit, Michigan.

A general meeting of the New York Academy of Sciences and its affiliated societies is announced for Monday, April 26, 1915, at the American Museum of Natural History. There will be a social hour, with refreshments, beginning at 9:30 p.m., preceded, at 8:15 p.m., by a lecture under the auspices of the Section of Astronomy, Physics and Chemistry, entitled "The Volcano Kilauea in Action," illustrated with lantern slides, by Dr. Arthur L. Day, director, Geophysical Laboratory, Carnegie Institution, Washington, D. C.

Dr. Victor C. Vaughan, professor of hygiene and preventive medicine in the University of Michigan, Ann Arbor, delivered an address at a special meeting of the College of Physicians of Philadelphia, on April 12, on phases of modern military hygiene and camp sanitation, particularly in reference to war mortality.

Professor Douglas W. Johnson, of Columbia University, delivered an illustrated lecture on "Surface Features of Europe as a Factor in the War," at Johns Hopkins University on April 8. On the preceding evening he addressed the Harrisburg Natural History Society on "The Origin of Scenery in the Grand Canyon District."

On April 16 Mr. R. J. Hammond, chemist of the U. S. Bureau of Mines, lectured at the University of Illinois on "The Radium Industry in America."

Dr. A. J. Carlson, of the University of Chicago, spoke before six hundred students in physiology at the Ohio State University on April 9. He chose as his topic "Some Recent Contributions to the Physiology of the Stomach." Dr. Carlson summarized his investigations, giving especial attention to the cause of hunger pangs. This was the final lecture in the annual series offered by the department of physiology to its students. Professor Carlson will address a joint meeting of the Alpha Omega Alpha Chapter, of the Western Reserve Medical School and the Section of Experimental Medicine of the Cleveland Academy of Medicine on May 14 at the Medical Library on "Some Recent Contributions to the Physiology of the Stomach."

A stated meeting of the Geographic Society of Chicago was held on April 9, when a lecture was given by Mr. Charles W. Furlong, of Boston, Massachusetts, the title being "Chile and the Fuégian Archipelago."

Professor Arthur E. Haynes, who for eighteen years held the chair of mathematics at the University of Minnesota until his retirement in 1911, died on March 12, at the age of sixty-six years.

Dr. Ernest P. Magruder, of Washington, D. C., one of the physicians at the head of the American Red Cross unit in Serbia, has fallen a victim to typhus fever. For the last five years before going to Serbia Dr. Magruder had been professor of clinical surgery in Georgetown University.

The death is announced from Berlin of Professor Friedrich Loeffler, the distinguished pathologist, who in 1884 discovered the diphtheria bacillus. Dr. Loeffler was born on June 24, 1852.

Dr. Arthur Sheriden Lea, formerly university lecturer at Cambridge, known for his researches in physiological chemistry, died on March 23, at the age of sixty-one years.

Professor Georg Jochmann, of Berlin, has died from typhus fever, contracted in one of the camps for Russian prisoners.

Dr. August Volkenhauer, docent for geology in Göttingen, has been killed in the war.

The *Journal* of the American Medical Association records deaths among foreign students of the medical sciences as follows: A. Birnbacher, professor of ophthalmology at the University of Graz, aged 66, an authority on glaucoma in particular, but best known, perhaps, by his operation for ptosis and for cataract and his method of illumination of the eye; J. D. Pinero, professor of anatomy at the University of Buenos Aires and chief of the sanitary inspection service of the port and of the vaccine service, member of the national board of health and physician in chief at the hospital for men; J. G. Rueda, president of the board of health for the province of Cordoba, Argentina, governor, and member of the national senate, aged 53; G. Resinelli, professor of obstetrics at the University of Florence, aged 50; H. Apolant, a coworker with Ehrlich at Frankfurt, aged 48; Kreisarzt Filgenträger, of typhus contracted at the Langensalza camp of prisoners; Otto Markus, assistant at the Würzburg medical clinic, killed by a shell during the Argonne fighting. He leaves unfinished an important work on the histology of the ganglion cells of the nervous system.

The next annual meeting of the American Psychological Association is set for December 28, 29, 30, at Chicago, Ill.

Governor Whitman has signed the Walters bill, which appropriates $50,000 for the eradication of the foot and mouth disease.

Governor Fielder has signed the bill giving to the State Board of Health the power to grant to regularly incorporated colleges,

universities and philanthropic institutions in New Jersey permission to make experiments on animals under certain restrictions. The Rockefeller Foundation for Medical Research will now begin work on the construction of a laboratory near Princeton for the study of animal diseases. The ground, buildings and equipment of the new laboratory will cost, it is estimated, $1,000,000. As has already been announced, Dr. Theobald Smith, professor of comparative pathology at Harvard, will direct the institution.

WE learn from the *Journal* of the American Medical Association that the Langenbeck-Virchow building, the new home for the medical and surgical societies of Berlin, is on the point of completion. The library is already being moved into the new quarters. By combining several scattered medical libraries, it starts with 113,000 volumes.

ACCORDING to a cablegram from Nish, dated April 11, the British and French governments are sending large numbers of military surgeons into Serbia to fight the epidemic of typhus. Thirty English surgeons have already arrived. Fifty French physicians arrived on April 10 and fifty more are expected shortly, as well as a party sent out by the Rockefeller Foundation and the American Red Cross.

THE *Journal of Criminal Law and Criminology* is entering upon the publication of a series of monograph supplements which will be known as Criminal Science Monographs. The first monograph is now in the press. It will appear early next fall under the title "Pathological Swindling and Lying." Dr. William Healy, of Chicago, is the author. The volume will approximate two hundred pages. Each number in this series will be bound in cloth, and will come from the press of Little, Brown and Co., Boston, Massachusetts. Persons who have manuscripts in hand or in preparation, which they wish to have considered for publication in this series should communicate with Professor Robert H. Gault, Northwestern University, Evanston, Illinois.

THE Prussian department of education has petitioned the legislature for a continuance of the appropriation of 25,000 marks, which for six years has been granted for cancer research, on condition that private subscriptions would double the amount. This has always been done, and the private subscriptions are already assured for 1915. The appropriation is devoted mainly to the cancer research work being done under Professor Ehrlich's supervision.

AN institution for ethnological research has been founded in Leipzig. The new institution forms part of the King Friedrich August Foundation for Scientific Research. It is affiliated with the Ethnographic Museum of Leipzig, and is furthermore in close connection with the Ethnological Seminar at the university. Dr. Karl Weule, director of the museum, is also director of the research institution. It may be expected that excellent results will be obtained by this concentration of effort, which contrasts favorably with the dispersion of energy as found in cities like Vienna and St. Petersburg and in most cities of the United States.

UNIVERSITY AND EDUCATIONAL NEWS

HARVARD UNIVERSITY receives $100,000 by the will of James J. Myers, of Cambridge.

GIFTS amounting to $72,908, to be devoted to cancer research at the Harvard Medical School, are announced. The sum of $50,000 was provided by the will of Philip C. Lockwood.

BY the will of Mrs. Laura L. Ogden Whaling, of Cincinnati, Miami University receives $250,000 for a dormitory with $10,000 for its support. $10,000 is bequeathed to the alumni loan fund. The residue of the estate is to be divided between Miami University and the Cincinnati Museum Association, and it is said that each institution may receive $200,000.

THE Addison Brown collection of plants offered to Amherst College by Mrs. Brown in memory of her husband, at one time a member of the class of 1852, has now come into possession of the college. Containing many thousands of specimens collected in the United States, Mexico, Porto Rico, the Hawaiian Islands and elsewhere, it is by far the largest accession ever received by the department.

PLANS have been drawn for the construction of four greenhouses, a heating plant, wells and windmills, and an underground piping system for irrigation purposes, on the new botanical garden for the department of botany of the University of Michigan. The old botanical garden east of the city with the 10,000 trees and shrubs which have been planted there, will be made into a tree and shrub park in about a year.

DR. HENRY C. COWLES, Dr. C. J. Chamberlain and Dr. O. W. Caldwell have been promoted to full professorships of botany at the University of Chicago.

DR. JULIUS STIEGLITZ, professor of chemistry and director of analytical chemistry in the University of Chicago, has accepted an invitation to give courses in chemistry at the University of California during the summer term that begins June 21 and closes on August 1.

PROFESSOR DANIEL STARCH, of the University of Wisconsin, will give courses in educational psychology and educational measurements at the University of Washington, Seattle, during the coming summer session.

AT the University of Birmingham Dr. Douglas Stanley has been appointed to the chair of therapeutics, and Dr. L. G. Parsons to a newly created lectureship in infant hygiene and diseases peculiar to children.

DISCUSSION AND CORRESPONDENCE

THE FUNDAMENTAL EQUATION OF DYNAMICS

THE difference of opinion between Professor Huntington and myself is probably less than might be inferred from his recent communication.[1] I do not object to the use of the equation $F/F' = a/a'$, which indeed is a useful one. But it seems to me misleading to call this *the* fundamental equation of dynamics, because there is something equally fundamental that is quite independent of this equation—the fact that the *mass* of a body is one of the factors determining what acceleration it has under the action of a given force. The same fact is expressed by Professor Huntington in the words[2] "different bodies require different amounts of force to give them any specified acceleration," which he refers to as "this central fact of dynamics." My view is that this "central fact" should receive explicit and quantitative[3] statement in whatever equation or equations may be adopted for expressing the fundamental law of acceleration. The principle which such equations must express may be stated in different ways. In the review[4] which called forth Professor Huntington's comment I expressed the opinion that the method most intelligible to the beginner is to introduce at the outset the body-constant which was called by Newton *mass* or *quantity of matter*, and to make the fundamental principle a statement of the way in which the acceleration of a body depends quantitatively upon both the applied force and the mass of the body. The principle then takes the following form:

(*a*) *A force acting upon a body otherwise free would give it, at every instant, an acceleration proportional directly to the force and inversely to the mass of the body.*

The meaning is perhaps more clearly brought out by writing a definite proportion:

(*b*) *Forces F, F', acting upon bodies whose masses are m, m', cause accelerations a, a' such that*

$$\frac{a}{a'} = \frac{F}{F'} \cdot \frac{m'}{m}. \tag{1}$$

It is instructive to consider the following partial statements of the general principle:

(*c*) *If the same body is acted upon at different times by forces F, F' and if a, a' are the accelerations caused, then*

$$\frac{a}{a'} = \frac{F}{F'}. \tag{2}$$

[1] SCIENCE, February 5, 1915.

[2] These words seem to be a very definite corroboration of the statement (quoted with disapproval by Professor Huntington) that "an equation which results from comparing the effects of different forces *upon the same body* can not be regarded as a complete expression of the fundamental law of motion; it is equally important to compare the effects of forces *acting upon any different bodies.*"

[3] The mere qualitative statement above quoted is no more satisfactory than the statement that "different forces acting at different times upon the same body cause different accelerations."

[4] SCIENCE, December 4, 1914.

(*d*) *If bodies whose masses are m, m′ are acted upon by equal forces, causing accelerations a, a′, then*

$$\frac{a}{a'} = \frac{m'}{m}. \tag{3}$$

(*e*) *If bodies whose masses are m, m′ are acted upon by forces F, F′ such that equal accelerations are caused, then*

$$\frac{F}{F'} = \frac{m}{m'}. \tag{4}$$

Equations (2), (3) and (4) are all particular cases of (1), but it requires two of them to express the whole import of (1), and there is no reason for regarding (2) as more fundamental or more important than (3) or (4). Any single equation which may properly be called *the* fundamental equation of dynamics must be equivalent in import to equation (1).

This does not mean that it is never allowable or advantageous to use the less general equations; on the contrary, problems and illustrations falling under these special cases are undoubtedly helpful to students. But the object should be to lead up to an understanding of the fully general principle expressed above in paragraphs (*a*) and (*b*) and in equation (1).

When this principle is fully understood, it is seen that equation (1) enables us to determine the acceleration of *any* body of known mass due to the action of *any* known force as soon as we know the acceleration of *one* body of known mass due to the action of *one* known force. For practical use it is advantageous to express the general equation in a more concise form. It is readily understood that (1) is equivalent to the equation

$$a = k\frac{F}{m}, \tag{5}$$

in which k is a constant of which the value depends upon the units chosen for expressing acceleration, force and mass; and that the still more concise form

$$a = \frac{F}{m} \tag{6}$$

results if units are so chosen that *unit force acting upon unit mass causes unit acceleration.*

The foregoing is essentially the Newtonian explanation of the second law of motion as interpreted by Thomson and Tait and accepted by other high authorities. In essential meaning there is no difference between this and the method advocated by Professor Huntington. The word mass is, indeed, avoided in his statement; but in recognizing the importance of the fact "that different bodies require different amounts of force to give them any specified acceleration" he recognizes the reality and fundamental importance of the body-constant which is usually designated as mass. By whatever name this constant may be called, it must play a part in the theory equivalent to that taken by mass in the equations given above. In Professor Huntington's presentation this part is taken by "standard weight," defined as the force required to give the body the acceleration 32.1740 ft./sec.[2] This does not conflict with the theory outlined above; in fact since the forces required to give different bodies a specified acceleration are by equation (4) proportional to their masses, standard weight as above defined may serve as a valid measure of mass. In explaining this method, however, it is important to make perfectly clear the fact that the quantity called standard weight is in reality the measure of a body-constant and is quite independent of gravity, in spite of the fact that it is given a name which is almost always associated with gravity. If properly safeguarded in this respect, Professor Huntington's method of developing fundamental principles is, I believe, logically defensible. Whether it meets the needs of beginners as well as that based upon the Newtonian treatment of mass may, however, be questioned.

To start with the notion of mass defined provisionally as "quantity of matter" has the same kind of advantage as starting with the "spring-balance" definition of force. Both definitions have a sufficiently definite meaning, gained from ordinary experience, to be of service in a preliminary explanation of the laws of motion. In comparing the masses of bodies composed of one homogeneous substance the significance of the words "quantity of matter" is indeed readily recognized, and it is distinctly helpful to generalize this notion even

though we must also recognize that any *general* method of comparing quantities of matter must employ either the laws of dynamics or some other physical law in which the same body-constant is significant.

Reflection upon what is really involved in the Newtonian laws soon shows, indeed, that the provisional definitions of force and mass are quite inadequate as a basis for a strictly logical explanation of the laws. It has long been recognized by writers who have attempted to formulate fundamental principles with full logical rigor that the definitions of both force and mass are implicitly involved in the laws of motion themselves.[5] An analysis of the strict logical import of the Newtonian system would, however, be quite unintelligible to beginners, and a recognition of the soundness of such an analysis is no reason for dispensing with the aid of the more tangible notions of *quantity of matter*[6] and *push or pull.*

While the method advocated by Professor Huntington is in my opinion sound in its essential features, the explanation of it in the paper[7] to which he refers seems to encourage the erroneous notion that the laws or facts of terrestrial gravity form a part of the principles of dynamics. Although the definition of standard weight quoted above is of course quite independent of gravity, in the paper it is given the form of a gravity definition: "The *standard weight* of a body is the force of gravity on that body in the standard locality." The reader is likely to miss the significance of the qualifying statement made elsewhere in the paper that the standard locality is any locality where g has the value 32.1740 ft./sec.^{2}—a statement which makes the reference to locality and to the force of gravity wholly irrelevant as regards the real meaning of the quantity called *standard weight.*

It is to be feared, also, that the definition of "force of gravity" given in the paper encourages vagueness rather than definiteness in the force concept. The conception of force as a *push* or *pull,* exemplified by the pull which stretches a spring, is a very definite one. It loses its definiteness, however, unless the fact is kept in mind that *there is always some body that does the pushing or pulling.* When, therefore, it is said that a body is acted upon by a certain force, it is always pertinent to ask *by what body this force is exerted.* How is this question to be answered in the case of the "force of gravity" as defined in the paper? The definition is as follows:

> By the "force of gravity" on a body, we mean simply the unseen[8] force which gives the body, when allowed to fall freely from rest, in vacuo, in the given locality, the observed acceleration g. It is equal and opposite to the force required to support the body in that locality.

The question *by what body this force of gravity is exerted* is not answered in the paper, and an attempt to supply the answer leads to the conclusion that the definition is inconsistent with the conception of force as *a push or pull exerted upon a body by another body.* The "observed acceleration g" has a component that is not due to force at all, but to the fact that our base for estimating acceleration is the rotating earth. The body is not acted upon by a push or pull that is "equal and opposite to the force required to support the body"; if it were, a supported body would have no acceleration, while in fact it has an

[5] Probably the most adequate formulation of the Newtonian laws from the point of view of strict logic is that given by W. H. Macaulay (*Bull. Am. Math. Soc.,* July, 1897). Mr. Macaulay's analysis makes it clear not only that the definitions of mass and force are implicitly contained in the laws themselves, but that the law of acceleration and the law of action and reaction can not be treated as independent, and further that the question of a base for estimating acceleration is of fundamental importance, since the laws, if true for one rigid base, will not be true for another which has any motion except a uniform translation with respect to the first.

[6] Professor Huntington's statement that the mass concept is "a derived concept, both historically and practically" is hardly true in any sense in which it is not also true of force. At all events mass in the sense of quantity of matter has been treated as fundamental by many high authorities from Newton down. See the opening paragraph of the "Principia."

[7] *Bull. S. P. E. E.,* June, 1913.

[8] Is the word "unseen" here intended to imply that there are forces which are visible?

acceleration even though at rest relatively to the earth.

Professor Huntington objects to the definition "force of gravity = attraction of the earth" on account of "complications connected with the spheroidal shape of the earth, the influence of the earth's rotation, etc." From what has been said above it is quite evident, however, that if the complications[9] connected with the earth's rotation are evaded by his definition it is only by a sacrifice of clearness in the force concept.

Clear thinking about the concept of force would seem to be promoted by the more usual method of distinguishing between *true* and *apparent* force of gravity; the former being the actual earth-pull on a body, the latter the pull or push exerted by a body upon its support. Each of these is a true force (a pull or push exerted by a specified body); to assume them equal is a first approximation to the truth. The reason they are not exactly equal can be explained rigorously when the student is in a position to understand the dynamics of circular motion; before that stage is reached it is sufficient to stop with the explanation which neglects the effect of the earth's rotation.

L. M. HOSKINS

[9] The spheroidal shape of the earth introduces no complication whatever as regards the definition "force of gravity = attraction of the earth." It is not necessary to be able to compute the attraction in order to understand the definition.

THE NATURE OF THE ULTIMATE MAGNETIC PARTICLE

FOR many years scientists have agreed in ascribing the magnetic properties of bodies to the action of exceedingly small elementary magnets, but the nature of these ultimate magnetic particles has been an open question. The influence of temperature, chemical composition and other factors has received the simplest explanation on the theory that molecules, or possibly groups of molecules, are the ultimate magnetic particles. On the other hand the electron theory of magnetism, developed by Langevin, Curie, Weiss and others, seems logically sound and is the only theory which has successfully accounted for diamagnetism.

The recently developed method of determining the positions of atoms within a crystal by X-ray photography and the ferromagnetic properties of magnetite, hematite and pyrrhotite crystals suggested a direct experimental method of eliminating one or the other of these two theories. By comparing photographs taken through these crystals while magnetized and unmagnetized it can be determined with certainty whether or not the atoms have moved from their positions of equilibrium during the process of magnetization. We have obtained experimental results with magnetite and hematite which indicate that the atoms do not leave their positions of equilibrium during magnetization. These results are consistent with the electron theory of magnetism and prove conclusively that magnetism is not a molecular phenomenon.

K. T. COMPTON,
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REED COLLEGE

THE NEW GLACIER PARK

TO THE EDITOR OF SCIENCE: Referring to the pleasing intelligence communicated by Dr. John M. Clarke, in SCIENCE for March 12, relative to the new glacier park near Syracuse, a further note on the history of its investigation may well be added. It would seem that the earliest clear interpretation of the glacial stream channels about Jamesville came from a master of physiographic study who has strewn many seed thoughts by the way during the past forty years—Mr. G. K. Gilbert. The record is in "Old Tracks of Erian Drainage in Western New York," an abstract published in the *Bulletin* of the Geological Society of America, Vol. 8, 1897, pp. 285–286. Dr. Quereau's paper, which appeared in the *Bulletin* of the following year, cites Mr. Gilbert's interpretation by way of acknowledgment, and both papers have been followed by the full expositions of Professor Fairchild in the publications of the Geological Survey of New York.

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SCIENTIFIC BOOKS

A Monograph of the Molluscan Fauna of the Orthaulax Pugnax Zone of the Oligocene of Tampa, Florida. By William Healey Dall, U. S. Nat. Mus., Bull. No. 90. Pp. xv, 173; Pl. 1-26. Jan. 21, 1915.

Collectors of curios and fossils alike have long known of the beautifully preserved specimens in the Tampa "silex beds" of Florida. But their chief interest, as the author of this monograph aptly remarks, "is not limited to their esthetic beauty, nor their position as characteristic of one horizon in the series illustrating the evolution of life on the globe, but is of extreme importance as furnishing a key to the little-understood succession of the Tertiary beds which fringe the islands of the West Indies and the encircling continental shores of Mexico, Central America and northern South America. The Tertiary column of the coastal plain of the Gulf states being fairly well elucidated, the relative position of the deposits to the south can be determined if any one of them can be satisfactorily connected with a given horizon in the North American series. Such a connection is afforded by the fauna of the silex beds of Tampa."

This problem confronting the paleontologists of the New World is strictly analogous to that presented Mesozoic and Cenozoic workers in the Old, viz., correlating northern temperate faunas with widely differing ones of a more southern, tropical character. There, much progress is being made of late in the older Tertiaries by the use of organisms other than molluscan, especially Foraminifera, and a similar tendency will doubtless soon be shown in this country. Yet there, such forms as *Velates Schmiedelianus* have served well as indices of horizons in both the southern and northern provinces of Europe, and such characteristic types as *Orthaulax pugnax* are equally serviceable here, from the Panama Canal to Georgia. Incidentally we may note the author's attempt to designate other horizons by characteristic species, as, for example, in the upper Oligocene:

Zone of—	*Former designation—*
Scapharca dodona	Alum Bluff beds
Cardium cestum	Chipola marl
Orbitolites floridanus	Tampa limestone
Orthaulax pugnax	*Orthaulax* bed

It is certain that only by the study of such faunal zones we may ever hope to be able to correlate the widely scattered Tertiary deposits of the West Indies and Central America.

As regards the relations of the fauna of the *Orthaulax pugnax* zone Dall says:

Four species go back as far as the Claiborne sands, six are found in the Jackson Eocene, and seven in the Vicksburg. Eight come up from the *Lepidocyclina* zone, four have been recognized in the scanty fauna known from the Nummulitic zone, and one or two from the very imperfectly explored Chattahoochee fauna. Eight are known from the Tertiary of Santo Domingo, several of which are very characteristic of the zone. The two characteristic species of *Orthaulax* occur in the lower Oligocene of the Panama Canal Zone, and at least one of them has been obtained in Santo Domingo, Antigua, and Anguilla.

"Above the *Orthaulax* zone we find 51 of its species surviving in the *Cardium cestum* zone, but only 14 reach the zone of *Scapharca dodona*.

"Fifteen occur in extra-Floridian Miocene beds, but only three in the Floridian Miocene; 11 are found in the Pliocene of south Florida, and five in the Florida Pleistocene, while 23 survive in the recent fauna." Of the 312 molluscan species known from the *Orthaulax pugnax* zone, 90 are described in this monograph as new, while about 120 are refigured from the author's well-known Wagner Institute papers. More than half the remaining species described by various authors are discussed and re-figured.

Of special interest in this generally marine assemblage of species is the presence of 27 land and fresh-water forms, consisting, among others, of *Helix*-like, Bulimoid, Pupoid and *Planorbis* types. One new genus is described, viz., *Microcerion* "about midway between *Cerion* proper and the small Pupidæ."

No sympathetic regard for generic names

that have been in use for many decades has prevented the author from relegating them to oblivion if some other name seem to him to be the correct one in accordance with the strict rules of biologic nomenclature. For examples, note the following:

Strophia changed to *Cerion, Vermetus* to *Vermicularis, Utriculus* to *Retusa, Pleurotoma* to *Turris, Fulgur* to *Busycon, Eulima* to *Melanella, Astralium* to *Astraea, Crassatella* to *Crassatellites, Cylindrella* to *Urocoptis, Tornatina* to *Acteocina, Bulla* to *Bullaria, Turbinella* to *Xancus, Tritonidea* to *Cantharus, Sigaretus* to *Sinum, Pectunculus* to *Glycymeris, Lucina* to *Phacoides.*

Since the above changes are for the most part mere substitutions of a less well-known name for one in more general use, there can be no doubt that it becomes the most of us with less special training in molluscan nomenclature to follow Dr. Dall's lead in our future publications. However, in some instances the changes suggested are based on Bolten's publication, referred to as "Mus. Boltenianum, 1798," antedating Lamarck in "Prodrome" by one year; yet of that rare edition we have understood Dr. Dall to say that there are but four copies in existence, though recently Schorborn's republication (75 copies) renders the work more accessible to workers, at least in the vicinities of large libraries. To what extent the old masters were excusable for not possessing one out of perhaps a half dozen copies of a private work seems to us certainly a legitimate query. Nor does the number 75 strike us as rashly great in this early twentieth-century literature. The only change we sincerely regret is *Pectunculus* to *Glycymeris,* both names having become well established in the literature for very different types from those now proposed. However imperative the inexorable laws of biologic nomenclature may be as regards this matter, Blainville's use in "Man. Malac., Vol. I., p. 540, 1825, of the adjective *Phacoides* can not be regarded as furnishing a sound basis for "Genus PHACOIDES Blainville." However, so far as the undersigned is concerned, such matters are very secondary in importance to the many statements and suggestions regarding matters of correlation and evolution. Note the artificiality of certain generic terms as brought out in Dall's discussion of the species *Vellorita floridana.* He says: "this fossil has the conchological features of the recent species, the *V. cyprinoides* of Asia, but the combination is one which is probably due to dynamic causes operating upon a species of *Cyrena,* and which might occur sporadically anywhere within the distribution of the genus *Cyrena.* The Asiatic or African forms have probably no more intimate connection with the American fossils than that thus indicated, and the same is true of the fossil *Batissa* from the Puget group and its South Sea analogues. The 'genus' *Hinnites* is another form in which it is unlikely that there is any genetic connection between the species occurring in different horizons except what is furnished by the genus *Pecten,* from which *Hinnites* species are probably mere sports."

Extremely valuable as a connecting link between the Jackson and Vicksburg forerunners and the Recent *Busycons* is the new species figured as *B. stellatum* (Pl. 10, Figs. 7, 9). Noteworthy from a lithologic standpoint is the statement that silicification of the calcareous matter of the fossils exposed between tides is now going on. We heartily agree with the author in his dislike of the present usage of the term "formation." We have never understood why the taxonomy of geologic units should be other than that suggested by the International Geol. Congress, '89, *i. e.,* Group, System, Series, Stage, with corresponding time units, Era, Period, Epoch and Age. Finally, as an interesting matter in methods of illustration, we have a chance to see in this monograph in juxtaposition some excellent pen-and-ink drawings by McConnell and the results of modern photographic methods in use by the U. S. Geological Survey. The latter are good, though sometimes showing a lack on the part of the artist of the finer essential features of the shell. This monograph must be regarded as a distinct and valu-

able contribution by America's foremost student of Cenozoic invertebrate paleontology.

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Text-book of Physiological Chemistry. By Olof Hammarsten. Translation by John A. Mandel. Seventh edition. Wiley and Sons, New York.

Another edition of Hammarsten's "Text-book of Physiological Chemistry" is now available in English (translation by Mandel).

Outside of the material contained in the first two chapters of the last edition which now have been combined into one, the arrangement of the older editions has been retained. Chapter for chapter, almost every subject treated occupies very nearly the same number of pages as before. Nevertheless, this edition is far from being a mere reprint. The newer observations and references are usually to be found—sometimes in place of older observations (and references), but more frequently as additions. In the field of metabolism, a field always somewhat scattered and submerged in "Hammarsten," the new edition will prove disappointing to American students just as the older editions have been. Most of the facts are there, but it takes a brave and diligent student to find them.

The index is very full, but its usefulness to students is not so great as it might be because it still lacks expert systematization. The first subject that the reviewer happened to look up in the index was mucin; sixteen references are given, the first is entirely misleading and the most essential references are tucked away in the middle of the long list.

Index and all, however, American biochemists are always pleased to see one more edition of the book which more than any other is kept within reach for daily consultation.

Otto Folin

NOTES ON ENTOMOLOGY

Two recent parts of Das Tierreich treating of the hymenopterous superfamily Proctotrypoidea[1] are almost monographic in character. Both are by the Abbey J. J. Kieffer and treat of almost 1,800 species in over 130 genera. An illustration is given of nearly every genus, and there is an introductory portion treating of the external morphology. Many of the species are from our country.

India is sufficiently distant from both Europe and America and its fauna has been sufficiently unknown to have been selected as the probable place of origin of many types of animals. Its insect fauna, however, is now becoming better known through numerous books; three have come to hand recently. One by T. B. Fletcher[2] deals with the insects of southern India. There is an introductory account of insects, and life histories of many species representing most of the families. A second large work is by E. P. Stebbing[3] and is devoted to accounts of the life history of and the damage wrought by the forest beetles of India. Unfortunately it contains the descriptions of various new species. The third work is purely economic and treats of the pests of various crops.[4] It consists of 84 leaflets with plates, mostly colored, of insect and fungous enemies of field crops.

Several microlepidopterists had shown that certain Tineid larvæ are of different shape and habit at different stages of development. Trågårdh[5] has investigated these forms and arranges them in two sections, the tissue eaters that bite and eat the parenchyma of the leaf, and the sap-feeders, that take only liquid. The former method is the more primitive, the

[1] Lief. 41, Bethylidæ, 595 pp., 205 figs.; Lief. 42, Serphidæ and Calliceratidæ, 254 pp., 103 figs., 1914.

[2] "Some South Indian Insects," Madras, 1914, 565 pp., 440 figs., 50 pls.

[3] "Indian Forest Insects of Economic Importance—Coleoptera," London, 1914; 648 pp., many pls. and text figures.

[4] "Crop Pest Handbook for Behar and Orissa," Calcutta, 1913. Issued by Dept. of Agric. of these provinces.

[5] "Contributions Towards the Comparative Morphology of the Trophi of the Lepiodopterous Leaf-miners," *Arkiv Zoologi*, VIII., No. 9, 48 pp., 62 figs., 1915.

latter requiring special modifications of the mouth parts. Several species are sap-feeders when young, and in later stages become tissue feeders.

The first impulse, upon finding some strange new form of insect, seems expressible only in a new ordinal name. Dr. Silvestri has found some small forms (2 mm. long) in Africa and Malasia representing the newest order of insects, Zoraptera.[6] The genus *Zorotypus* is based upon several species resembling young Gryllidæ. They have enlarged hind femora, two jointed tarsi, head with distinct Y mark, no eyes, last joint of palpi enlarged, nine-jointed antennæ, and short one-jointed cerci.

It is indeed refreshing to find a paper on the systematics of Culicidæ that contains no new generic names. Mr. E. Brunnetti[7] has studied the proposed genera of mosquitoes from the standpoint of the dipterologist and comes to the conclusion of Dr. Williston that most of these names are based on characters of no generic value in Diptera, and are therefore synonyms. Under *Culex* he places no less than 72 such names. The value of the various characters is considered, and tables are given to the vaild genera; some names, however, still left in doubt. *Corethra* is regarded as forming a subfamily in the Culicidæ.

We all know that an insect "bite" is not simply a puncture, but our first interest has been to find a remedy. Dr. J. H. Stokes[8] however, has investigated the pathological and histological features of a "bite" and concludes, that, irrespective of pathogenic organisms, the insect introduces a toxic agent which produces considerable changes in the tissues near the "bite." This toxic agent is not injured by alcohol nor by dry heat, but is inert after treatment with hydrochloric acid. The history of a "bite" is divided into four stages; the papular, the pseudovesicular, the vesicular or oozing stage, and the involution or subsidence.

N. BANKS

[6] "Descrizione di un nuovo ordine di insetti," *Bol. Lab. Zool. Gen. Agrar.*, VII., pp. 193–209, 1914.

[7] "Critical Review of 'Genera' in Culicidæ," *Rec. Ind. Mus.*, X., pp. 15–79, 1914.

[8] "A Clinical, Pathological and Experimental Study of Lesions Produced by the Bite of the Black Fly (*Simulium venustum*)," *Jour. Cutan Diseases*, November and December, 1914, pp. 46.

SPECIAL ARTICLES

A CASE OF ASSUMPTION OF MALE SECONDARY SEX CHARACTERS BY A COW [1]

A PURE-BRED registered Ayrshire cow, named Dorothy of Orono (23010), belonging to the University of Maine, produced three calves, on dates as follows: September 17, 1909, September 10, 1910, February 24, 1912. On the lactation following the birth of the second calf she made a record of 12,426.4 lbs of milk and 450.75 lbs. of fat, and was admitted as No. 426 to the Ayrshire Advanced Registry.

After March 24, 1913, the cow never gave any milk. The udder rapidly shrunk to a very small size and the animal began to show the external characteristics of a bull. This change was very slight at first, but soon became much more marked. *After a lapse of 8 months the general external facies and the behavior of the cow were like those of a bull to a remarkable degree.* The neck had become thickened in its posterior parts, and had developed a well-marked crest, as is characteristic of a bull. If the cow had been so screened that only her fore-quarters and neck were visible, any observer would have unquestionably pronounced her a male. The assumption of male characters in these regions was complete and perfect. In the hind-quarters the change from characteristic female conformation in the male direction, while less striking than in the anterior parts, was still clearly evident. The udder shrunk away to a very small size. The hips and rump took on the smooth, rounded, filled-out appearance which is characteristic of the bull, but not of the cow.

The cow was slaughtered on February 18, 1914. Autopsy showed as the only gross ab-

[1] This is a preliminary abstract of a paper having the title "Sex Studies. VII. On the Assumption of Male Secondary Characters by a Cow Affected with Cystic Degeneration of the Ovaries," shortly to be published in the Annual Report of the Maine Agr. Expt. Sta. for 1915.

normality a simple cystic condition of the ovaries. Histologically and cytologically these cystic ovaries differed from the normal cow's ovary in but one essential respect, namely, that they had no corpora lutea.

The case described presents for consideration certain definite and clear-cut results bearing on the problem of secondary sex characters. These are:

1. This cow had been a perfectly normal female and had performed all the reproductive functions, both primary and secondary, of the sex.

2. It later assumed certain of the secondary characters of the male, both in respect of structure and behavior, with perfect definiteness, and, so far as the characters concerned go, completeness. This change was, for example, at least as complete and definite as any of those described by Steinach[2] following castration and transplantation of gonads.

3. The gonads of this animal, examined subsequent to the change in secondary characters, were exactly like those of a normal cow, save in the one respect that the follicles were not breaking and discharging ova, but were forming follicular cysts or becoming atretic, and because of this no corpora lutea were formed.

(*a*) The interstitial secreting mechanism of these ovaries was absolutely normal, both in respect of number of cells, and the cytological characteristics of the individual cells.

(*b*) The germinal mechanism was perfectly normal up to the point where ovulation should occur. Then it failed to separate the ova from the ovary.

(*c*) The outstanding, and so far as we can determine the only significant, anatomical and physiological difference between the gonads of this abnormal cow and those of a normal one, consists in the fact that the former lacked any lutear tissue.

A detailed account of the case, with figures, will be given in the complete paper.

RAYMOND PEARL,
FRANK M. SURFACE

[2] Steinach, E., "Willkürlich Umwandlung von Saügetiermännchen in Tiere mit ausgeprägt weiblichen Geschlechtscharacteren und weiblicher Psyche," *Pflüger's Arch.*, Bd. 144, pp. 71–108, 1912.

A NEW THEORY REGARDING THE FEEDING POWER OF PLANTS[1]

THE feeding power of plants has been a subject of a great deal of investigation during the last half century. Undoubtedly mere casual observation of the growth of wild and cultivated plants led investigators long ago to surmise that there is a difference in the feeding power of different species of plants. Numerous carefully controlled experiments have repeatedly confirmed this idea. Of the important mineral elements needed by plants, sufficient phosphates in an available form are most often lacking in a soil. It is largely on this account that phosphates have generally been used in testing the feeding power of plants. Fortunately phosphates are also well adapted to this study. With the rapidly increasing use of phosphate fertilizers, the subject has become one of considerable economic importance, since it may be possible that with a proper selection and sequence of crops as regards their feeding power, the cheap insoluble phosphate fertilizers may be used with greater advantage.

It was formerly supposed that insoluble minerals were made soluble by plants through the action of various acids secreted by the plant roots. As is well known, later experiments, especially those by Czapex, have demonstrated that other than carbonic acid, plants normally excrete at the most, only minute traces of acids. There remains, however, no question that practically all plants excrete through their roots large quantities of carbonic acid. Lately some investigators have suggested that differences in feeding power may be due to differences in amount of carbonic acid excreted by the roots. Experimental data, however, lend little support to this view, and hence indicate that there must be something vastly more important in determining the feeding power of a plant. On reviewing the literature concerning the subject, and considering the data obtained in this laboratory, the writer was led to formulate the following hypothesis:

Plants containing a relatively high calcium

[1] Publication authorized by the director of the Wisconsin Experiment Station.

oxide content have a relatively high feeding power for the phosphorus in raw rock phosphate. For plants containing a relatively low calcium oxide content the converse of the above is true. A calcium oxide content of less than one per cent. may be considered relatively low. Corn, oats, rye, wheat and millet belong in this class. A calcium oxide content of somewhat more than one per cent. may be considered relatively high. Peas, clover, alfalfa, buckwheat and most of the species of the cruciferæ belong in this class.

The explanation of the above relation is made possible by means of the laws of mass action and chemical equilibrium. The reaction making the phosphorus in raw rock phosphate available to plants is one between carbonic acid and the tricalcium phosphate in the rock phosphate, which may be represented as follows:

$$Ca_3(PO_4)_2 + 2H_2CO_3 \leftrightarrows Ca_2H_2(PO_4)_2 + CaH_2(CO_3)_2.$$

As is well known if none of the products to the right of the reaction are removed from solution, the reaction soon reaches a state of equilibrium. If the di-calcium phosphate is continually removed but the calcium bi-carbonate only in part, then the reaction will continue a little farther, but also soon comes to a state of equilibrium due to the accumulation of the calcium bi-carbonate. When this point is reached, the further solution of the phosphate is prevented. This is the condition that obtains for such plants as are low in calcium oxide and hence do not absorb the calcium bicarbonate in the proportion to the dicalcium phosphate as given in the reaction. In such cases, the plants soon suffer for soluble phosphates. If both of the products to the right of the reaction are simultaneously and continually removed in the proportion given, then the reaction continues from left to right and there results a continuous supply of soluble phosphates along with soluble calcium bicarbonate. This is the condition that obtains, at least in part, with plants containing a high calcium oxide content, and hence such plants are strong feeders on raw rock phosphate.

In accord with other investigators the writer has found that the use of ammonium nitrate or sulfate as a source of nitrogen in quartz plant culture work, greatly increases the availability of raw rock phosphate to plants which are normally weak feeders on this material. In the light of the present theory this is very satisfactorily explained as follows: Calcium bicarbonate being much more soluble in a water solution of ammonium salts than in water alone, it follows that the addition of ammonium salts allows the preceding reaction to continue from left to right to a much greater extent than if water alone is present. The addition of a salt in which the products of the reaction are more soluble has the same effect to a certain extent as is obtained by removing the products of the reaction.

With the theory[2] here proposed it is possible to predict from the calcium oxide content of a plant whether or not that plant in quartz cultures will be a strong or weak feeder on raw rock phosphate. Under soil conditions there are many subsidiary factors that influence the availability of phosphates, and hence under such conditions the relative growth of a plant can not be taken rigidly as a true index of its feeding power for the limiting element which is supplied in an insoluble form. Seeming deviations from the theory may result under such conditions. It is possible that with proper restrictions the theory can be applied to the feeding power of plants in a broader way, involving the use of other insoluble plant-food materials besides rock phosphate, and the general theorem could then be worded as follows: The feeding power of a plant for an insoluble substance depends primarily upon two conditions, viz., (1) the solubility of that substance in carbonated water and, (2) whether or not the plant removes from solution all the products of the solubility reaction in the proper proportion, so as to allow the solubility reaction to continue indefinitely.

With the theory here presented the writer

[2] Since writing this article the writer's attention has been called to a recent publication in *Zhur. Opytn. Agron.*, 15 (1914), No. 1, 54, by F. V. Chirikov, who from entirely independent work from this, has come to practically the same conclusion as the one set forth in this paper.

believes that the feeding power of plants is satisfactorily explained, without the intervention of other acids than carbonic. Since the failure to establish that plants excrete notable amounts of other acids than carbonic, some investigators, as previously stated, have suggested that the differences in feeding power may be due to differences in amount of carbon dioxide excreted. A careful consideration of available data lends little support to this idea. It seems rather that it is the efficiency with which the carbon dioxide is used, and not the differences in amount excreted by different species of plants, that determines whether or not a plant will feed strongly on an insoluble material.

The writer has in preparation a detailed article dealing with the feeding power of plants and the availability of phosphates.

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THE SOCIETY OF AMERICAN BACTERIOLOGISTS[1]

SYSTEMATIC BACTERIOLOGY

Under the supervision of H. A. Harding

A Study of B. subtilis by Means of the Classification Card: H. Joel Conn.

One hundred and thirty cultures of the *B. subtilis* type, isolated from soil, have been studied by means of the classification card adopted by the society. The definition adopted for *B. subtilis* is: a large, peritrichic, spore-producing rod, facultative anærobic in the presence of dextrose, liquefying gelatine, and growing vigorously on ordinary media without chromogenesis, producing a membraneous more or less wrinkled growth on the surface of agar. Two questions have been considered: (1) Do the determinations called for on the card separate these 130 cultures into more than one species? (2) Does the same culture always give identical results upon repetition of the tests?

In answering the first question half of the determinations represented by the "Group Number" on the card were excluded because they are implied by the definition of *B. subtilis*. The determinations taken into account were the fermentation of sugars and glycerin, and the reduction of nitrates. The nitrate reduction determination gives quite clear-cut results and may serve to separate an infrequent nitrate-negative species from an abundant nitrate-positive species. The fermentation tests do not give such definite results. They suggest that the 130 strains do not differ from each other in fermentative powers, but give inconstant results with the present technique.

The second question was answered in the negative as regards the fermentation tests; the nitrate reduction test seemed more constant, but insufficient data is at hand to settle the matter.

These tests indicate that with our present technique different "group numbers" do not always indicate different species. One of the first steps needed in revising the card is to establish the best methods for making the various determinations.

Some Induced Changes in Streptococci: Jean Broadhurst.

Various relatively simple physical and chemical factors (such as changes in temperature and differences in artificial media) differ greatly from such agents as saliva, intestinal extracts, and pure cultures of other bacteria, in their effects upon the physiological activities of selected strains of streptococci. The physiological effects of the former, especially in the various test media containing the sugars and the related substances suggested by Gordon, are mainly of a negative or inhibiting type, and apparently temporary only.

The changes induced by the latter factors (saliva, intestinal extracts, etc.) are, however, markedly different. They are changes in kind not in amount of reaction; they are active and usually include new powers, not merely the inhibition or occasional stimulation of earlier powers or capabilities, and often indicate a complete rearrangement of the fermentative complex. These induced changes have, so far, been practically permanent.

A Study of the Correlation of the Agglutination and the Fermentation Reactions among the Streptococci: I. J. Kligler.

Bacteria have evolved so little along gross structural lines that it is impossible to differentiate members of the same genus on a merely physical basis. We therefore resort to the more delicate criteria of protoplasmic constitution and physiological activity, in which direction remarkable differentiation exists. Tests for the finer structural differences of these organisms are found in their behavior to differential stains, such as the Gram stain, and to the immune substances in-

[1] Abstracts of papers presented at the Philadelphia meeting, December 29, 1914.

duced by them in the animal body. Their physiological activity is measured by the end products of their metabolism. Physiologically, bacteria, generally, have evolved in two main directions—one group possessing marked carbohydrate splitting properties, the other having developed the property of digesting various protein substances. The streptococci belong to the former division, showing but little tendency to proteolysis.

It appears natural enough to assume that the biologic activities of a cell would correspond with the chemical nature of its protoplasm. Yet such a correlation has not been worked out except in a few isolated cases. Among the streptococci such a correlation, if it exists, would be especially significant in that it would help to differentiate the various members of a genus that has puzzled many investigators.

The agglutination, fermentation and hemolytic properties of sixty strains of streptococci obtained from various pathological conditions, were studied, using four agglutinating sera having a titer of 800–1,000, and six carbohydrates and other fermentable substances as follows:

Disaccharides	Lactose
	Saccharose
Trisaccharide	Raffinose
Alcohol	Mannite
Glucoside	Salicin
Starch	Inulin

Only a limited number of the strains were agglutinated by the sera used. A definite correlation was, however, obtained between the agglutinative and fermentative characters. The serum produced by a strain of one fermentative group (the group that fermented salicin, for instance) agglutinated only cultures of its particular division and failed to agglutinate members of any of the other groups. No such correlation was obtained with the hemolytic property, members of one hemolytic group being agglutinated by the sera produced by strains from other hemolytic groups.

The results obtained indicate that a separation of the streptococci obtained from various pathological conditions into three fermentative types would coincide most closely with their natural relationship.

The groups suggested are:

1. Salicin fermenters only, generally hemolytic —*Str. pyogenes.*

2. Raffinose fermenters, salicin usually fermented, mannite always negative, generally produces a green colony on blood agar—*Str. salivarius.*

3. Mannite fermenters, generally ferment salicin, rarely ferment raffinose, variable in their reaction to blood—*Str. fecalis.*

The Filterability of B. bronchisepticus: with an Argument for a Uniform Method of Filtration: N. S. Ferry.

The purpose of the paper was to place on record a series of filtration experiments with *Bacillus bronchisepticus,* described as the cause of canine distemper by Ferry in 1910, McGowan in 1911 and Torry in 1913.

The experiments were conducted as follows: The organism was grown twenty-four hours both on agar and in bouillon. The bouillon growth was filtered, undiluted, while the agar growth was taken off in bouillon and made into a suspension of about the same density as the bouillon culture. The method of testing the integrity of the filters was that described by Bulloch and Craw in the *Journal of Hygiene* in 1909, which depends upon the measure of the pressure of air as it is allowed to pass through the pores of the candles while immersed in water. The filtration was conducted at room temperature, one hour taken as the length of time for filtration, and three pressures were used; gravity, 15 lbs. (negative) and 225 lbs. (positive).

The experiments proved conclusively that the *Bacillus bronchisepticus* is a filterable microorganism. The work also corroborates the results of previous investigators with regard to the fact that the less pressure used the more easily will some organisms pass through the filters.

Some interesting possibilities were suggested by the outcome of this work. Since 1905, when Carre claimed he had produced typical symptoms of distemper in susceptible dogs from the filtered discharges of diseased dogs, the majority of writers have classified the etiology of canine distemper as a filterable, invisible or ultramicroscopic virus, and it is so described in many textbooks. The work of Ferry, McGowan and Torry with the *Bacillus bronchisepticus* tends to refute the statements of Carre.

The results of the present filtration experiments puts an entirely new light on the subject. If the *Bacillus bronchisepticus* is the cause of canine distemper, then the experiments corroborate the work of Carre. If the work of Carre was correct, and if the causative agent of canine distemper is a filterable virus, then the experiments point very conclusively to *Bacillus bronchisepticus* as the etiological factor and confirms the findings of the three previously mentioned investigators.

The author suggests that steps be undertaken for making out some uniform method of conducting filtration experiments of testing the efficiency of the candle and expressing or recording the results.

Influence of the Concentration of the Nutrient Substrate upon Microorganisms: ZAE NORTHRUP.

1. *Determination of the Influence of the Concentration of the Gelatin in Nutrient Gelatin, upon Liquefying and Non-liquefying Organisms.*—Gelatin media, having the same amount of other nutrient substances than gelatin, per unit volume were prepared, using 15 per cent., 25 per cent., 35 per cent., 50 per cent. and 75 per cent. gelatin.

Difficulties were met in the preparation of the highest percentage of gelatin on account of the thick sticky nature of the mass, but an excess of water was added to make the mixture homogeneous, this water being then driven off by evaporation on a water bath.

Pure cultures of both liquefying and non-liquefying organisms were plated on the different concentrations of gelatin.

On account of the extreme viscidity of the 75 per cent. gelatin it could not be plated in the usual manner; a thin film of the gelatin was spread over a sterile glass slide in a sterile petri dish and inoculated by spreading a small drop of a 24-hour culture of the organism on the surface of the gelatin.

The number, size and appearance of colonies were to be noted on the media of the respective concentrations.

In counting, the low power of the compound microscope ocular No. 1 and objective No. 7 was found to give counts 3–4 times as high as the ordinary counting lens.

The numbers of organisms developing on the plates are influenced to some but not to any marked extent, if the mechanical difficulties of inoculating the gelatin and pouring the plates are taken into consideration. The decrease or variations noted may be due only to experimental error.

The size of the colonies was found to be inversely proportional to the concentration of the gelatin. This was especially marked in the case of the organisms which are the most active in liquefying gelatin.

The type and appearance of the colonies were also found to be worthy of note. The subsurface colonies of both liquefying and nonliquefying organisms appeared like very fine gas bubbles distributed throughout the medium. The active liquefying organisms began to show a rectangular instead of a concave depression in surface colonies on 35 per cent. and 50 per cent. gelatin, while with the slow liquefier a new type of growth, a stalagmite-like or apiculate growth, appeared on the 50 per cent. gelatin. This type of growth was noted in the 25 per cent. gelatin of colonies of the non-liquefying organisms.

B. typhosus was the only organism among the eight types used in the experiment which refused to grow on the 50 per cent. gelatin. However another trial might prove successful.

The different phenomena observed in the course of this experiment will most probably call upon the sciences of physics and of physical chemistry for their interpretation.

Several questions have been called forth by the results of this experiment and most of them remain as yet unanswered.

What part does the medium or substratum and what part does the organism play in the formation of the so-called characteristic growths which are obtained in solid media? What force or forces cause the variation in types of liquefaction produced by various proteolytic enzyme-forming organisms? Does the inherent nature of the organism or its secretions play the greater part or are physical or physico-chemical forces the greater factor?

Why is the size and the structure of the colony so markedly influenced by the media of increasing concentration? It is not due to osmotic pressure, as gelatin is a colloid and consequently will exert no osmotic pressure.

Is it due to the lack of water or is it due to some physical property of the gelatin, as surface tension, which is more evident in greater concentrations?

What force causes the colony in a nutrient gelatin of high concentration to show a rectangular depression when in ordinary nutrient gelatin the depression is concave?

In the liquefaction of ordinary nutrient gelatin what part does the force of gravity play?

An interesting occurrence was noted in the "plates" made with the 75 per cent. gelatin. Upon examining these plates, several days (exact period of time not noted) after they were made, the glass slides were found in very fine pieces as if crushed by a powerful force. This occurred in every case. The crushing of the slide was evidently due to the contraction of the highly concentrated gelatin upon cooling and solidification. Just how much energy it will take to crush slides by mechanical force is yet to be determined.

The determination of the various physical and physico-chemical forces will serve to give some idea of the factors which microorganisms have to overcome in growing in gelatin and similar media of high concentration.

This experiment was worked out by Mr. O. M. Gruzit, a senior student.

Induced Variations in Chromogenesis: M. R. SMIRNOW.

Of the various biological characters of bacteria, one of the most interesting yet least important is that of pigment production. Though considerable efforts have been expended in the study of this function, little of real value is as yet available. It appears that this property is especially prominent amongst the saprophitic organisms, and depends to a greater or less extent, on certain conditions of environment which vary with different bacteria, and is, as a rule, more or less constant for the same organisms.

With the exception of *Spirillum rubrum* and possibly a few others, the chromogenic bacteria require an abundance of free oxygen, giving no pigment under anærobic conditions of growth. Temperature also seems to determine pigment production of some bacteria, thus the *B. prodigiosus* will give no pigment at 37° C.

Perhaps the most important influencing agent on the function of chromogenesis is the medium on which the organism is grown. With other factors of environment constant, chromogenesis will vary with the medium employed. Gessard, for instance, has shown that the *B. pyocyaneus* will produce only a blue color, of a most beautiful shade, in a two-per-cent. solution of peptone, which may be increased in intensity by the addition of five-per-cent. of glycerin. When grown on egg-white or other albumen or on weak glucose media it would produce a fluorescent green. This same organism when grown on a five- or six-per-cent. glucose medium or on immune serum would give no pigment. He believes that phosphates are required for the production of the fluorescin.

Substances that enhance the value of culture media, in a general way increase also the pigment production. Other substances, as acids or alkalies, may diminish or even inhibit its production. Some organisms may give different colors on media of different reaction. Thus the *B. prodigiosus* gives a distinct yellow color on alkaline, and a violet-red on acid media.

In what manner the pigment is produced is not yet known. It is regarded that the property of pigment production keeps pace with other biological characters, as enzyme formation. This, the writer does not feel to be correct, inasmuch, as will later be shown, he has succeeded in increasing the chromogenic properties of some bacteria with a coincident decrease of enzyme formation. Some of the higher forms of organisms give rise to pigment as a function closely related with their nutrition and may possibly be regarded as products of metabolism. In these cases the pigment is obtained from the medium and is stored up in the bodies of the cells, as in the case of sulphur bacteria. Or, it may be produced on certain media containing iron, as evidenced in the so-called iron bacteria, through the products of metabolism and the production of sulphide or iron.

Chromogenesis may be increased not only by growing the bacteria on more favorable media and environment, but also by the process of selection, transplanting each time from portions of the culture or from a colony that shows the most pronounced pigment.

Experimentally induced variations in the chromogenic properties of the *Staphylococcus pyogenes aureus* may be brought about by exposure to phenol or by growth in phenol, glucose, sodium sulphate or sodium chloride broth. Nine different strains of the *Staphylococcus* were used in the work here reported. Five of these were old stock cultures giving little or no color; the remainder were a few months old and showed a fair amount of pigment at the beginning of the experiments.

The organisms were grown in the above media for from six to ten weeks, being transplanted every three or four days during the entire time. They were then grown on potato and blood serum media for from 24 to 120 hours, and the effect on chromogenesis noted.

The increase of chromogenesis is brought about more readily by growing the organisms in phenol broth than by exposing them to 75 per cent. phenol solution and transferring on to agar. Of the nine strains used phenol markedly increased the chromogenic properties in six, Nos. 1, 2, 5, 6, 7, 8; slightly increased it in Nos. 4 and 9 and left No. 3 practically unchanged or even slightly diminished. Growth in dextrose, sodium chloride and sodium sulphate broth invariably decreased or left unchanged the quantity of pigment produced. Often almost a pure white growth of the various cocci, subjected to the growth in NaCl and Na_2SO_4 broth, would be seen when transferred to potato or blood serum.

An old stock culture of the *B. prodigiosus* was also used. This organism gave the slightest trace

of color at 20° C. at the beginning of the experiment. The organism was subjected to phenol only, beginning with a few minutes' exposure to a 0.75 per cent. solution and increasing as it became more resistant up to fifty or sixty minutes. Cultures were made on agar and grown at 20° C.

A striking increase in color production on all media resulted, the color becoming deeper and deeper until the maximum was reached at the thirteenth exposure. Up to the nineteenth exposure the color of each succeeding growth became most pronounced in 48 hours. From thereon, with increasing time exposures, the color production was slower, the color reaching its maximum in three or four days. Different shades of red were produced on different media. On agar, the color was deep brick red; on blood serum it had more of a scarlet hue; while on potato the color was somewhat variable and not as marked as on the other media. It was, however, on glycerin agar and glycerin potato that the most striking results were observed. The original strain gave no color on glycerin agar, and only a pale, delicate reddish color on glycerin potato. Transplants made from the phenol exposed organisms gave a brilliant cherry-red color on glycerin potato spreading to surround the entire surface of the medium. On glycerin agar, a dull cherry-red color was obtained.

In summing up what has been said concerning chromogenesis, it becomes evident that this faculty is more or less closely associated with the metabolic activities of bacteria, nutritive or otherwise. It varies with the strain and is more or less dependent on oxygen, temperature, and the medium used. An organism may produce more than one color at once and the same time or it may produce different colors, depending upon environment and the medium used, particularly the latter. Finally, chromogenesis may be varied through the agency of chemicals, as seen by the work here outlined, phenol generally increasing, and glucose, sodium chloride and sodium sulphate diminishing this function.

Induced Variations in the Cultural Characters of B. coli: M. R. SMIRNOW.

The same technique that was used in the experiments on chromogenesis was made use of here. In all, 21 different strains of the various bacilli of the colon-typhoid group were used, but this report is confined only to the *B. coli,* of which seven different strains were experimented on. All of these strains were obtained from the Museum of Natural History of New York through the kindness of Dr. C.-E. A. Winslow, and were the stock Nos. 19, 44, 45, 46, 52, 57 and 95. The transplanting was carried out every three or four days over periods varying from one to three months, thus allowing from ten to thirty or more transfers. The results obtained in each set of experiments were rather constant, though not altogether so, inasmuch as some of the strains reacted quicker or different in the degree of the action at one time than another.

Control cultures were carried on in plain broth throughout the experiment. It might be stated at once that there were very slight variations between the original stocks and these control cultures, but no more than would be expected as normal variations. These were seen as slightly increased or decreased amounts of gas or acid formation, in time of coagulation, or slight changes in the growth on potato. At no time, however, were the biological characters markedly changed nor enzyme production completely inhibited simply by continual passage through broth.

Growth on Potato.—Dextrose seemed to have a special effect upon the character of growth of *B. coli* on this medium. Five of the seven strains showed at best only a slight yellow or a very light brownish growth on ordinary potato, with practically no discoloration of the medium. Very frequently, indeed, the dextrose-affected organisms would give the typical "invisible" growth seen with the *B. typhosus.* Both the original stock and the control showed the characteristic colon growth on this medium. This change was noted so many times that the explanation based on differences in the composition of the potato could be excluded. Three of these five strains also showed this change after exposure to phenol. One strain of the *B. coli,* not changed in this respect with either dextrose or phenol, showed this same variation after growing in either sodium chloride or sodium sulphate broth.

Action in Milk.—Both phenol and dextrose diminished the acid production and inhibited the formation of lab enzyme in three of the seven strains of the *B. coli,* either entirely or for a period of two weeks at least. These results were not seen with the use of the strong saline or sodium sulphate broth.

Fermentation of Sugars.—The results obtained with these substances on *B. coli* with reference to variations in sugar fermentations can be best seen in the accompanying charts. The most striking changes here also were seen in those organisms

exposed to dextrose and phenol. The former completely inhibited both acid and gas formation and all the sugars tested in three different strains. In two other strains dextrose varied the fermentation of the sugars as to amount of acid and gas formation, some of which were totally inhibited. Phenol inhibited these fermentations in all of the sugars in only one case, and in four other strains it at times diminished this reaction to the point of inhibition. Sodium chloride and sodium sulphate had less effect than did phenol, giving usually slight variations in amount of acid or gas produced with an occasional inhibition.

Inhibition of all the sugar fermentations in any one experiment was almost always accompanied by inhibition in the usual changes in milk, the characteristic growth on potato, and the formation of indol.

Variations in Indol Production.—The production of indol is regarded by many bacteriologists to be as important a biological characteristic of *B. coli* as its fermentations of the sugars, and is even thought to be of greater importance in its differentiation. This quality, however, under normal conditions, varies considerably in its quantity and time of appearance with most strains, and at times requires more delicate tests than the usual Salakowsky method for its determination.

In the experiments here reported it appears that of the variations induced in *B. coli* that of indol production is the first to take place, often disappearing in the third or fourth culture in dextrose broth. This does not hold however when the bacteria grow in the other media, as evidenced below.

Each strain of *B. coli* was grown in plain broth as control, in dextrose, phenol, sodium chloride and sodium sulphate broth and on potato. Thirty-five sub-cultures were made in all. Indol was tested for after the 10th, 15th, 25th and 35th transfers. The tests for indol were made by inoculating one loop of culture from the respective media to which each strain was subjected into standard peptone solutions, grown for seven days at 37° C. and then tested by the Salakowsky method. All the tests were done at the same time, using the same batch of peptone solution throughout the experiment.

All the controls, grown in plain broth gave good indol tests even after the 35th sub-culture. Those grown in dextrose broth gave none at the 10th subculture nor thereafter. In phenol broth the property of indol production seemed to be somewhat increased, judging from the intensity of the reaction. Sodium chloride and sodium sulphate and prolonged cultivation on potato practically exerted no influence, or if any, showed but a slight inhibitory effect.

Experiments were then carried out to see how soon the property of indol production is interfered with by growth in three per cent. dextrose broth, and it was found that *B. coli* lost this property usually on the third and at times on the second transfer over a period of from seven to ten days. In one experiment sub-cultures were made every 24 hours, with a total disappearance of the indol tests in 48 or 72 hours in all the strains.

In order to exclude the possibility of interference in the indol test by the presence of three per cent. dextrose, several cultures in plain broth, also peptone, were made and grown at 37° for seven days. Dextrose was added to each of the cultures and then tested for indol. Positive tests were obtained in all, hence excluding any possibility of such interference by the presence of the carbohydrate.

Experiments were then carried out to determine the permanency of this change. The cultures in dextrose broth after the 35th transfer were taken and grown in plain broth, transplanting every day and tested on the seventh day of incubation. Four of the strains of *B. coli*, Nos. 44, 45, 46 and 52, gave slight indol reactions on the third transfer, No. 46 gave a good positive on the fifth transfer, but the others took five to ten more transfers before they could be called "+" or "++" positive. Nos. 57 and 95 took six transfers before a trace of indol appeared. No. 19, a very feeble indol producer in the control, remained negative up to the fifteenth transfer, at which time the experiment was discontinued.

In summing up then, it can be said that dextrose and phenol, particularly the former, cause partial inhibition or total disappearance of acid and enzyme formation in some strains of *B. coli*. These changes, together with the suspension of the production of indol and the characteristic colon growth on potato, make the *B. coli* approach if not entirely appear like the *B. typhosus* type organism. These changes have been noted time and again, but in varying degrees, in those strains that are susceptible to variations, but for some unexplained reason can not be regarded as altogether constant. Indol formation would invariably return when these altered bacteria were transplanted into plain broth at frequent intervals. Lab enzyme would also return in most of the altered strains, but not invariably so. The same can be said of the fermentative properties, but even to a less extent. Very often, however, these characteristics appear to be entirely done away with, the change

being permanent as far as could be made evident by sub-culturing into plain broth. In these cases observations were made up to two months after the last exposure to the influencing substance, making frequent transfers. There seemed to be no definite rule of reversion, no constant results and no relation between the reappearance of one enzyme and another. The reappearance of the fermenting enzymes in one sugar was not necessarily accompanied by those in other sugars. At times the fermentation of one sugar might have returned to nearly normal, while others might show little or no presence of gas with the same strain of *B. coli*.

Halophytic and Lime Precipitating Bacteria: K. F. KELLERMAN AND N. R. SMITH.

Of approximately 70 cultures isolated from the Great Salt Lake and from sea water from Florida and the Bahamas three types of organisms were secured. *Pseudomonas calcis*,[2] a new spirillum and a new bacterium were isolated from the sea water. Closely similar varieties of species of *Spirillum* and *Pseudomonas* were found in water from the Great Salt Lake. Both in sea water and in the Great Salt Lake these bacteria are associated with the precipitation of calcium carbonate.

Relation of Crop to Bacterial Transformation of Nitrogen in the Soil: K. F. KELLERMAN AND R. C. WRIGHT.

Progress report upon continuation of work reported[3] previously.

The Influence of Hydrogen-ion Concentrations upon the Physiological Activities of Bacillus coli: WM. MANSFIELD CLARK.

Attention is called to the importance of hydrogen-ion concentration for the physiology of cells and to to its importance for the solution of various problems of bacteriological chemistry. The experiments of Michaelis and Marcora upon the limiting hydrogen-ion concentration for *B. coli* have been elaborated and it is shown that although minor differences exist there is a limiting concentration at or above which all activity ceases. The same results were obtained with various cultures of the true colon bacillus. At the limiting hydrogen-ion concentration proteolysis is inhibited. With increase in temperature the effect of hydrogen-ion concentration increases. The relation of this fact to the so-called thermal death point is pointed out.

An example is given showing the usefulness of the hydrogen-electrode in bacteriological research. By a study of the reaction of the medium at the close of the fermentation it was shown that by the use of *p*-nitro phenol a separation of the colon arogenes family could be accomplished. The groups so separated were rigidly correlated with the gas ratio.

Bacteria of the Colon Type Occurring on Grains: L. A. ROGERS, WILLIAM MANSFIELD CLARK AND ALICE C. EVANS.

In an earlier paper it was shown that the colon bacteria of bovine feces belong to a very sharply defined type which was characterized by the production of a relatively small amount of gas composed of hydrogen and carbon dioxide in almost exactly equal parts. A study of the gas production by 166 colon-like cultures from grains as determined under carefully controlled conditions showed that these cultures could be divided into three physiological groups. These were (1) cultures giving a low volume composed of carbon dioxide only; (2) those giving a low volume and a carbon dioxide-hydrogen ratio of 1.06 and (3) those giving a high volume and a ratio varying from 1.90 to 2.90. The cultures producing a carbon dioxide only were also distinguished by the rapid liquefaction of gelatin. The low-ratio cultures, although agreeing with the fecal type in the gas production, were distinguished by the production of a yellow pigment. The 151 high-ratio cultures were divided into four types. Ninety of the 151 liquefied gelatin slowly, gave a carbon dioxide-hydrogen ratio of 2.50 to 2.80, produced a light cadmium pigment, failed to form indol from tryptophane, fermented saccharose and glycerine, and failed to ferment starch, inulin and adonite. Forty cultures failed to liquefy gelatin, gave a gas ratio of 2.20 to 2.50, and produced a light cream-colored pigment, did not produce indol from tryptophane, fermented saccharose, lactose and raffinose, but almost always failed to ferment the other test substances.

Two other groups, differing in their gas ratio and fermentation reaction were made but they included a relatively small number of cultures.

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(*To be continued*)

[2] Kellerman, Karl F., and Smith, N. R., "Bacterial Precipitation of Calcium Carbonate," *Jour. Washington Academy of Sciences*, Vol. IV., No. 14, August 19, 1914, pp. 400–02.

[3] Kellerman, K. F., and Wright, R. Claude, "Mutual Influence of Certain Crops in Relation to Nitrogen," *Journal American Society of Agronomy*, Vol. 6, 1914, pp. 204–10.